Vol. XVIII, No. 1 PSYCHOLOGICAL REVIEW PUBLICATIONS January, 1911

# THE Psychological Review

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THE REVIEW PUBLISHING CO.,
41 NORTH QUEEN ST., LANCASTER, PA.
AND BALTIMORE, MD.

Agents: G. E. Stechert & Co., London (2 Star Yard, Carey St., W. C.); Leipsic (Hospital St., 20); Paris (76 rue de Rennes).

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879.

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JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*Review*).
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PUBLISHED BY

THE REVIEW PUBLISHING CO.,

41 NORTH QUEEN ST., LANCASTER, PA.

AND BALTIMORE, MD.

AGENT G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.), LEIPZIG (Hospital St., 10), PARIS (76 rue de Rennes).

VOL. XVIII. No. 1. January, 1911.

# THE PSYCHOLOGICAL REVIEW

## SOME PROPERTIES OF THE FREE ASSOCIATION TIME[1]

BY FREDERIC LYMAN WELLS, PH.D.,

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The present discussion grows out of a body of some 12,000 observations, though all of them are not analyzed in the same detail. A general survey of a series of 100 associations with each of 25 subjects, 10 men and 15 women, makes up the first part of the report; the remainder of the experiments, in which 8 subjects, 1 man and 7 women, are concerned, are presented only with reference to specific questions of interpretation. The stimulus words of the first 2,500 observations are those of the Kent-Rosanoff standard series; grateful acknowledgment is here made of the coöperation of these writers in this portion of the study. Of the men subjects, two are physicians and the remainder nurses; the women subjects are nurses. Presentation and response were oral; in timing, the stopwatch was used.

The experiment which forms the subject of these remarks has come to be regarded from two very different points of view, and made to serve two very different purposes. In both free and controlled forms it has been the vehicle of a mass of investigations on the intimate nature of the mental process, as represented in the researches of Watt, Messer, Wreschner and others; while in different hands, the personal factors in the free association experiment have been more emphasized,

[1]From the Psychological Laboratory of the McLean Hospital.

and the attempt has been made to determine the elements of individual difference that could be measured by this means. What one gets out of this experiment may be partly a matter of technique, but it is certainly also a question of point of view. It is remarkable that the great body of work centering about the association test discontinuously with the papers of Jung and Riklin should have contained so little suggestion of those applications which they so fruitfully developed. Investigation from either of these view-points may be scientific or the reverse; it is largely a matter of temperament which view-point one finds the more interesting, though there should scarcely be the same uncertainty of which is the more directly useful. No apology seems necessary for adhering in this discussion strictly to the development of the association experiment as a method of individual psychology.

The essential routine of this familiar test is, of course, to have the operator speak a stimulus word, to which the subject is to reply with the "first thing it makes him think of," the "first word it suggests to him," "das erste Wort das klar zum Bewusstsein kommt." In effect, this is an agreement on the part of the subject to make some connoted reply to a series of intimate questions. And in spite of all instructions to the subject about the "very *first* word that comes into your mind," etc., and the best of coöperativeness, it is naïve to suppose that we get this response from the ordinary subject save in a comparatively small number of cases. For actual progress in the interpretation of the free association test we must abandon the idea of any such objective determination of the response, and, through a study of the experimental material at all angles, endeavor to answer the query, what conditions the responses, when the instructions of the free association test are given?

It is quite necessary to study, through introspection, the subjective processes in individual associations and one can fill any number of pages describing them; but in the last analysis, the *quickness* with which the response is elicited depends essentially upon four things, which might be rated in the order of their importance about as follows: (1) The tendency of the

associations to be presented in linguistic form, or in imagery readily resolvable thereto; (2) the relative predominance of some single association; (3) the willinginess or unwillingness, from any cause, to utter the response word that presents itself; (4) such direct inhibitory effect upon all voluntary response as may be induced by any strongly emotional reaction to the stimulus word. It is understood that these factors are not mutually exclusive; any or all may frequently operate together.

Strictly speaking, not all, nor perhaps the greater number of responses in the experiment can be designated as 'free' associations. There is very apt to be more or less forced suppression, rejection, and selection. The time required to form objectively adequate associations is often considerably shorter than the actual response time. The simpler controlled associations, such as the opposites, are uniformly shorter than the uncontrolled. It requires distinctly less time to give the opposite of *slow* than to give the first thing it makes you think of. In the latter case, while the word may arouse no associations of special affective moment, there is yet likely to be some striving for the mastery among the various associations that do present themselves, with no deciding factor as in the opposites test. It is seen here how completely the association test fails as a measure of the speed of the thought processes, since it is inconceivable that a particular class of associations should, on an average, be faster in arousal than any association.

In the free association test, any response in language may satisfy the conditions of the experiment. In the controlled association tests, usually but few responses are so capable. In the first case, the subject may make any response that he pleases; in the second, only a particular response. Thus, while giving the freedom of choice over the whole range of language, the test also imposes a responsibility of choice. In the controlled association test this freedom, and consequently the difficulty of choice, is reduced to a minimum. Here the response is chosen for us, and we have only to find it. In free association we have not only to find a suitable response, but also to choose it, and consent to it.

Special difficulties that make for a lengthened free associa-

tion time have already been enumerated. All of the shortest association times may be regarded as comparatively 'free.' In a strict interpretation, we can perhaps say also that all 'free' associations are short. The term 'free' association is certainly a misnomer when applied to those cases where there has been active and voluntary suppression or 'dodging'; these associations are controlled, or 'censored,' *sit venia verbo*, as truly as those of the opposites test; only the control is variable, recognized only by the subject, and, as in Langfeld's experiments, negative rather than positive. Such association times are almost certain to be long. It is probably fair to say that in the great majority, perhaps in all, association times above the median, there has been an inhibition, rejection, a suppression of some sort; such a process is normally frequent in the experiment, though it may occur from very different causes, or be dictated by very diverse motives. It is by no means necessary that they be associated with special emotional complexes.

Nor does association with a special emotional complex necessitate a lengthened time. What is apt to cause the lengthened time in these cases is not the presence of the complex, but its suppression. If there is no motive for suppression of the ideas there is no reason why the time should be longer, unless the subject be so distracted with special interests aroused by the stimulus word that the attention required for the formulation of the response is impossible.

From what we know of the speed of the thought processes in controlled association, where all such interferences are comparatively absent, it would seem that the time of strictly free association, progressing without interference, should normally range from considerably less than 1,000 sigma up to possibly 2,000 sigma; and a time considerably in excess of this limit indicates an interfering process from any or all of the above-named sources.

From this postulate it follows, that in length and especially in the variability of the association times, one may estimate not only the liability to such interference in different classes of stimuli, but also the liability to such interference as a temperamental characteristic in different individuals.

It is not surprising to find that association time tends to be shorter in educated individuals than in less educated ones. But the individual variations are considerable, and perhaps it would be better to say the individuals with short times are more apt to be educated. Over the range of normal variability, short free association time is in but negligible degree a function of education in the individual, just as it is not a measure of the speed with which his thought processes can go on. The shortest association times I have seen have been in individuals whom, though far from illiterate, one could scarcely rank in the higher levels of education. Moreover, within a given group, the individual differences in association time may be enormously greater than the differences in education. The median association times vary a great deal more than the shortest times, which range from five to seven fifths in all subjects, and those individuals have the shortest reaction times who have the capacity for reacting to the experiment in the freest way. In some individuals nearly all reactions are comparatively free, in others but very few indeed.

To illustrate, the entire first series of experiments with twenty-five subjects may be quoted as follows:

| MEN. | | | WOMEN. | | |
|---|---|---|---|---|---|
| Order. | Median in Fifths. | Smallest Number of Steps Including 50 % of the Cases (50 % Range). | Order. | Median in Fifths. | Smallest Number of Steps Including 50 % of the Cases (50 % Range). |
| I. | 7.7 | 2.7 | I. | 7.2 | 2.2 |
| II. | 7.8 | 2.8 | II. | 7.6 | 2.6 |
| III. | 8.0 | 3.5 | III. | 8.2 | 3.3 |
| IV. | 8.6 | 3.2 | IV. | 8.3 | 3.5 |
| V. | 8.6 | 3.6 | V. | 8.8 | 3.9 |
| VI. | 8.7 | 4.1 | VI. | 9.3 | 3.4 |
| VII. | 10.7 | 4.9 | VII. | 9.5 | 3.6 |
| VIII. | 10.8 | 5.2 | VIII. | 10.4 | 3.7 |
| IX. | 11.8 | 5.0 | IX. | 12.8 | 7.0 |
| X. | 13.3 | 5.8 | X. | 13.4 | 7.4 |
| | | | XI. | 14.1 | 8.6 |
| | | | XII. | 14.8 | 9.0 |
| | | | XIII. | 16.3 | 9.3 |
| | | | XIV. | 19.0 | 12.3 |
| | | | XV. | 22.0 | 13.6 |

In this table, the slowest subject takes about three times as long as the fastest, but considering that the zero point can scarcely be less than two fifths of a second, the range of indi-

vidual differences should hardly be taken as less than 4:1. The distribution of the median times is seen to be skewed toward the short end; the majority of subjects tend to approximate the limit, though others trail out to a considerable distance from it. There is no significant correspondence between the length of these times and the individual's education. The only way in which such figures can be justly interpreted is in the light of the subject's tendency to react to the experimental conditions in a decisive or unobstructed way. The association time must be regarded as a volitional rather than as an intellectual measure, indicating the tendency of the individual to make prompt choices and to act promptly upon them under the experimental conditions imposed. It measures the 'freedom' with which the subject responds.

The next point of contact between the results and this interpretation, lies in the general sex difference in association time. It has been variously observed that free association time is shorter in men than in women, but the above figures may give further suggestions as to the nature of this difference. A greater facility of response is actually found in individual women than in any of the men and the generally shorter time of the men results from their individual differences being less, and their generally closer approach to the limit.

It is not difficult to reasonably account for the way in which this result is produced. In a number of similar series of experiments[1] it has been the writer's experience that the behavior of the women under the experiments varies a great deal more than that of the men. Expecially is this true where personal or affective moments enter in the experiment to any extent. There is probably no recognized procedure in experimental psychology that involves these elements in a stronger degree than the association test. It is thus quite in harmony with previous experience that the range of individual differences is distinctly greater in the women than in the men, more so than could be accounted for in the greater number of women subjects.

In respect to this fact, as well as the generally longer times

[1]Cf. 'Sex Differences in the Tapping Test,' *Am. J. Psych.*, XX., 353–363.

of the women, it must not be overlooked that in the experiments resulting in the above generalization that women have longer association times, the experimenter has been a man. It would be by no means unnatural if these differences were much lessened, if not indeed reversed, in similar experiments made by a woman.

The sex differences noted in the median of the association time are much accentuated in the variability of the distributions given by the different subjects. This is given in the last column of the table, the 50 per cent. range. The sex differences in the range are here proportionately much greater than in the central tendency of the reaction time. In some ways, this figure has more logical significance than the median reaction time. To a number of words the subjects all react with a fair degree of promptness; it is the number of associations which are relatively obstructed, that is the important point of difference. It is here that there occurs the hesitation and indecision that results in long time and high variability, and which is seen to be so much more marked in the women than in the men.

It is more accurate to say in some women than in any of the men. For it is seen that the women fall into two distinct species here; the first eight individuals having a very small variability, below the central tendency for the men, while the remaining seven are much above it. The results of the first group are fairly commensurate with those seen in the men; the results in the second group are markedly different from those of the men. Considered as functions of temperament, the differences of experimental attitude indicated in such a grouping are not without interest.

The obvious task is to pursue these differences further, and to trace how far they may be related to the character of the responses. I have elsewhere outlined a classification[1] based upon that of Jung and Riklin which while far from final, seems to be the best that I can at present apply. Some difficulties and uncertainties of such classification have been mentioned, and certainly not overstated. It is often most

[1] 'Practice Effects in Free Association,' *Am. J. Psych.*, XXII., pp. 1–13.

difficult to decide into which of two or even more categories an association ought to be put. Indeed, this difficulty affects so directly the fundamental distinction of 'internal' and 'external' associations that it has seemed wiser to give up this distinction altogether. The reason for this, however, is pretty definite, and the general system of classification can still be preserved by making it more detailed. The 'internal' coördination (*Beiordnung*, not Jung's *Koordination*) is often indistinguishable from the 'external' coexistence, while the internal and external contrasts are absolutely continuous; it is simply a matter of the opinion as to what constitutes *'Gelaüfigkeit.'*[1] What has been done, therefore, is to include the contrasts in one group, and on the other hand to separate the *Koordination* of Jung into coördination, subordination and supraordination, since it is the first alone of these three that is so apt to be confused with coexistence. The distinction between the two is made as best it may be, but it is not nearly so well defined as others. In spite of the continuity, it has also seemed wiser to make a separate group for the more egocentric predicates. These are the most important reactions in the results from the psychodiagnostic standpoint, and show individual differences that are worth preserving.

It is well not to be too hide-bound by grammatical considerations. Little purpose can be served by a classification that calls *violet-lovely* a predicate and *violet-loveliness* a coördination. Both have the content of *Urteilsreaktionen*, and in the absence of contrary introspection should be recognized as such.

The following table will indicate the extent to which the sex differences in association time are correlated with differences in the form of the association. The table gives (1) the per cent. of associations falling into each class in all groups of subjects, (2) the median times of the associations in each class, (3) the 50 per cent. range of variability in these times.

[1]Nor is it altogether just to speak of 'mediate' responses as a restricted and minor category; as many as 50 per cent. of the responses may be 'mediate,' though falling, in their direct relation to the stimulus-word, into very different associative categories.

| | Per Cent. of Frequency. | | Median Time. | | 50 Per Cent. Range. | |
|---|---|---|---|---|---|---|
| | Men. | Women. | Men. | Women. | Men. | Women. |
| 1. Failure of response | None | *None* | ... | ... | .. | .. |
| 2. Direct egocentric | None | *None* | ... | ... | .. | .. |
| 3. Egocentric predicate | 1.4 | *2.5* | 18.0 | *15.2* | 8.0 | *8.3* |
| 4. Judgment of quality | 5.0 | *13.3* | 12.6 | *12.6* | 7.5 | *6.0* |
| 5. Simple predicate | 2.7 | *4.9* | 11.5 | *12.8* | 4.5 | *5.8* |
| 6. Subject-relation | 1.2 | *1.3* | 11.3 | *10.0* | 6.3 | *4.0* |
| 7. Object-relation | 2.1 | *2.9* | 9.0 | *10.6* | 5.0 | *4.5* |
| 8. Causality | 1.1 | *1.9* | 11.0 | *10.0* | 5.0 | *3.5* |
| 9. Coördination | 22.2 | *13.7* | 10.8 | *10.9* | 4.6 | *4.5* |
| 10. Subordination | 4.3 | *4.5* | 12.2 | *12.0* | 5.2 | *4.4* |
| 11. Supraordination | 17.7 | *11.5* | 10.3 | *11.9* | 4.3 | *6.0* |
| 12. Contrast | 15.3 | *15.5* | 8.3 | *9.1* | 2.5 | *3.3* |
| 13. Coexistence | 9.6 | *12.3* | 12.6 | *13.3* | 3.9 | *8.6* |
| 14. Identity | 12.2 | *10.3* | 10.6 | *13.0* | 4.6 | *7.8* |
| 15. Language-motor | 2.3 | *2.8* | 13.0 | *11.6* | 6.0 | *5.0* |
| 16. Word-compounding or completing | 2.0 | *1.7* | 11.0 | *11.0* | 5.0 | *5.0* |
| 17. Pure sound associations | .1 | *None* | 10.0 | ... | .. | .. |
| 18. Syntactic change | .8 | *1.0* | 11.0 | *15.5* | 4.0 | *7.5* |

The third, fourth and fifth categories, representing predicates and *Urteilsreaktionen*, are about twice as common in the women as in the men. This difference is quite consistent; the highest number reached by the men but little exceeds the average of the women. On the other hand, there is no corresponding difference in the association times, these being practically equal. The subject-relations, object-relations, and causalities perhaps occur slightly oftener in the women; the balance of the time difference is slightly in favor of the men, but the cases are throughout very few in number. A marked difference is seen in the consistently greater number of coördinations that the men assign, but there is no difference in the association time. The subordinates are practically equal both in frequency and in time. The supraordinates are again much more frequent in the men, and from here on the men begin to have distinctly shorter times. The contrasts are about equal in frequency, and it is worthy of note that in both men and women they have shorter times than any other category. The coexistences seem a little more frequent in the women, the identities in the men, the men

having shorter times in each case. In accord with other experience, the remaining categories are very scantily represented, and the times, even were the differences significant, would not influence the general results in any appreciable degree.

The shorter time of the men, therefore, is expressed in their quicker reactions in the associations of supraordination, contrast, coexistence and identity. This finding, together with the excess of the higher predicates in the women, probably harmonizes with the results of Jung and Riklin regarding the internal and external categories.

The figures of these results, more minutely analyzed, are very much against the supposition of a long or short reaction time as characteristic of an individual with a predicate, concrete, or other association type. Extremes of long and short reaction time may be found in extremes of either type, though there is little question that, in the long run, the times would average greater in those individuals who have many predicates. Altogether the most striking correlation of this nature concerns the supraordinates and the contrasts. There is strongly negative correlation between these two; a subject is apt to have many supraordinationes and few contrasts, or few supraordinates and many contrasts. The latter individuals not unnaturally have the shorter reaction times but it would be an error to infer from this that their generally shorter times are due to this abundance of contrast reactions. For in the first place, the difference between the supraordinate times and the contrast time is itself far too small to account for this difference between the groups, and secondly, the shorter times in these individuals are by no means confined to the contrasts. It seems more probable that those individuals possessing the inherent faculty of short association time, are more apt to react in this way. The correspondence, though not exceptionless, is quite consistent, being as follows:

The one markedly contrary case is the third subject among the women, though the fourteenth is also exceptional. If the subject has a short reaction time there are likely to be more contrasts than supraordinates; if a long one, more supraordinates than contrasts.

| Men. | | | Women. | | |
|---|---|---|---|---|---|
| Median Time of 100 Associations. | Number of Superordinates. | Number of Contrasts. | Median Time of 100 Associations. | Number of Superordinates. | Number of Contrasts. |
| 7.7 | 6 | 30 | 7.2 | 4 | 30 |
| 7.8 | 17 | 19 | 7.6 | 9 | 30 |
| 8.0 | 20 | 20 | 8.2 | 37 | 2 |
| 8.6 | 3 | 27 | 8.3 | 7 | 12 |
| 8.6 | 6 | 30 | 8.8 | 7 | 32 |
| 8.7 | 7 | 17 | 9.3 | 1 | 31 |
| 10.7 | 29 | 1 | 9.5 | 2 | 27 |
| 10.8 | 30 | 6 | 10.4 | 6 | 14 |
| 11.8 | 33 | 1 | 12.8 | 18 | 4 |
| 13.3 | 26 | 2 | 13.4 | 5 | 2 |
| | | | 14.1 | 19 | 2 |
| | | | 14.8 | 12 | 1 |
| | | | 16.3 | 8 | 3 |
| | | | 19.0 | 14 | 24 |
| | | | 22.0 | 23 | 18 |

A query which suggests itself in connection with these species in the contrast reactions is whether the forms of association that are infrequent in an individual tend to have longer or shorter times than those which are more common in him. With the contrasts, those who habitually resort to them have shorter times in them than those who do not, but elsewhere it is by no means the rule. While there are individual cases of very long reaction time in associations alone in their class, no significant correlation is here shown statistically. No general rule can be formulated for the order of association times in the different categories. The contrast associations, for example, are generally shorter than the identities, but in several individuals they are longer. It does not seem possible to analyze these differences further. Beyond such generalizations as that, by and large, coördinates are likely to be shorter than coexistences, and such special phenomena as characterize single individuals, reaction time and logical category are practically independent.

There is no reason why associations of especially affective moment should not take any of the forms mentioned, from failure of response to repetition of the stimulus,[1] especially where dodging is concerned; though the most apparent ones

[1] "By heaven, he echoes me,
As if there were some monster in his thought
Too hideous to be shown." *Othello.*

naturally fall into the category of the egocentric predicates. The longer times in this category are fully accounted for by this consideration.

In the foregoing, the experiment has been discussed almost exclusively from the standpoint of central tendencies, and with reference to the difference between individuals, or groups of subjects. An adequate conception of the picture can scarcely be given without considerable attention to the experiment from the standpoint of the individual stimulus words. One must not dispense with the minuter discussion of individual reactions, unfit as such treatment may be to establish general principles. The obvious danger of such a treatment is that in the quotation of what may be especially glaring instances of *Komplexwirkung* or what not, an exaggerated or distorted idea of the experiment is apt to be given. Against sensational interpretations of the experiment it is to be hoped that the previous remarks will serve to guard; yet in any endeavor to see the forest in spite of the trees, it is well not to entirely avoid seeing the trees because of the forest.

Previous investigators have examined the relation between the reaction-time and the grammatical categories of the stimulus words. The results do not altogether agree, but there is some tendency for the abstract nouns to be longest and for the adjectives and concrete nouns to be relatively short. In the Kent-Rosanoff series there are no verbs that are not also nouns; in central tendency, the concrete nouns are shortest, the adjectives next, and the abstracts distinctly longest. The two shortest words of the hundred are *bitter* and *ocean*, each with a median reaction time of 6.9; the longest word by many steps is, not insignificantly for the psychoanalysts, *wish*. Its reaction words are as follows:

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| desire | 36 | would | 32 | request | 18* | thinking | 13 | think | 13 |
| grant | 22 | have | 10 | thought | 18 | wanting | 25 | want | 7* |
| have | 65 | granted | 21 | uncertain | 23 | gratified | 13 | happiness | 34 |
| fancy | 32 | granted | 8 | want | 18 | idea | 32 | desire | 10* |
| money | 22 | true | 8 | comfort | 21 | gratify | 25 | hope | 9 |

All times save the three marked with an asterisk are above the median reaction time of the subject that gave them.

The words of the entire list have been arranged in the order of their median times, and this order will perhaps have sufficient intrinsic interest to warrant its printing in full,

| Order. | Word. | Median of 25 Observations. | Smallest Number of Steps Including 50 % of the Cases (50 % Range). | Order. | Word. | Median of 25 Observations. | Smallest Number of Steps Including 50 % of the Cases (50 % Range). |
|---|---|---|---|---|---|---|---|
| 1. | Bitter..... | 6.9 | 1.9 | 51. | Priest..... | 10.5 | 4.2 |
| 2. | Ocean..... | 6.9 | 2.2 | 52. | Bible ..... | 10.5 | 4.5 |
| 3. | White..... | 7.2 | 2.5 | 53. | Mountain .. | 10.5 | 4.5 |
| 4. | Dark ..... | 7.5 | 2.7 | 54. | Fruit ..... | 10.5 | 4.5 |
| 5. | Yellow..... | 7.7 | 2.7 | 55. | Music..... | 10.5 | 5.5 |
| 6. | Swift ..... | 7.7 | 2.8 | 56. | Rough ..... | 10.5 | 6.1 |
| 7. | Thirsty .... | 7.8 | 2.6 | 57. | Thief ..... | 10.5 | 6.2 |
| 8. | Sour..... | 7.8 | 3.5 | 58. | Boy ..... | 10.7 | 3.8 |
| 9. | Stove..... | 7.9 | 3.1 | 59. | Deep ..... | 10.7 | 4.7 |
| 10. | Scissors .... | 8.2 | 3.2 | 60. | Lion..... | 10.7 | 4.7 |
| 11. | Black..... | 8.2 | 3.2 | 61. | Heavy ..... | 10.9 | 5.7 |
| 12. | Girl ..... | 8.2 | 3.7 | 62. | Head ..... | 11.1 | 4.4 |
| 13. | Man..... | 8.3 | 2.8 | 63. | Loud ..... | 11.1 | 4.9 |
| 14. | Eagle..... | 8.4 | 3.4 | 64. | High..... | 11.1 | 6.1 |
| 15. | Long..... | 8.4 | 3.4 | 65. | Window.... | 11.1 | 6.1 |
| 16. | Blossom.... | 8.4 | 3.8 | 66. | Carpet..... | 11.2 | 3.8 |
| 17. | Sweet..... | 8.5 | 3.5 | 67. | Working ... | 11.2 | 4.2 |
| 18. | Hammer ... | 8.7 | 2.7 | 68. | Stem ..... | 11.2 | 4.2 |
| 19. | Sheep..... | 8.7 | 2.9 | 69. | Hungry .... | 11.2 | 5.2 |
| 20. | Blue..... | 8.7 | 3.6 | 70. | Cold..... | 11.2 | 5.2 |
| 21. | Spider ..... | 8.7 | 3.7 | 71. | Afraid ..... | 11.2 | 5.2 |
| 22. | Table..... | 8.7 | 3.7 | 72. | Street..... | 11.2 | 6.2 |
| 23. | Bath..... | 8.7 | 3.8 | 73. | Hard ..... | 11.5 | 5.5 |
| 24. | Cottage.... | 8.7 | 4.8 | 74. | Smooth .... | 11.5 | 5.5 |
| 25. | Soft..... | 8.8 | 3.5 | 75. | Soldier..... | 11.5 | 6.2 |
| 26. | Mutton .... | 8.8 | 3.7 | 76. | Child ..... | 11.5 | 6.5 |
| 27. | City..... | 8.9 | 3.5 | 77. | Chair ..... | 11.5 | 6.5 |
| 28. | River..... | 9.1 | 3.7 | 78. | Butter ..... | 11.5 | 6.5 |
| 29. | Lamp..... | 9.2 | 2.7 | 79. | Sleep ..... | 11.5 | 6.5 |
| 30. | Baby ..... | 9.2 | 4.2 | 80. | Memory ... | 11.6 | 4.6 |
| 31. | Needle..... | 9.2 | 4.2 | 81. | Butterfly... | 11.6 | 4.7 |
| 32. | Bed ..... | 9.4 | 3.6 | 82. | Cheese..... | 11.6 | 5.5 |
| 33. | Bread..... | 9.4 | 3.6 | 83. | Foot..... | 11.6 | 5.5 |
| 34. | Joy..... | 9.4 | 4.2 | 84. | Dream..... | 11.6 | 6.2 |
| 35. | Green..... | 9.4 | 4.4 | 85. | Comfort.... | 11.8 | 5.7 |
| 36. | Whistle .... | 9.5 | 3.7 | 86. | Quiet ..... | 12.5 | 6.5 |
| 37. | Woman .... | 9.5 | 4.5 | 87. | Whiskey ... | 12.5 | 6.5 |
| 38. | Sickness.... | 9.6 | 3.8 | 88. | Salt ..... | 12.5 | 6.5 |
| 39. | Earth..... | 9.6 | 3.9 | 89. | Citizen..... | 12.5 | 7.5 |
| 40. | Hand..... | 9.7 | 4.2 | 90. | Trouble.... | 13.5 | 7.5 |
| 41. | Red ..... | 9.7 | 4.7 | 91. | Square..... | 13.5 | 8.5 |
| 42. | Tobacco.... | 9.7 | 5.2 | 92. | Command .. | 13.7 | 6.5 |
| 43. | Eating..... | 9.8 | 3.5 | 93. | Anger..... | 14.2 | 7.2 |
| 44. | King..... | 9.8 | 4.5 | 94. | Religion.... | 14.2 | 8.5 |
| 45. | House ..... | 9.9 | 3.5 | 95. | Moon..... | 14.2 | 9.2 |
| 46. | Doctor..... | 9.9 | 4.7 | 96. | Slow..... | 14.5 | 9.5 |
| 47. | Beautiful... | 10.2 | 4.2 | 97. | Health..... | 14.5 | 10.5 |
| 48. | Short ..... | 10.2 | 5.2 | 98. | Justice..... | 15.5 | 7.7 |
| 49. | Light ..... | 10.2 | 5.5 | 99. | Stomach ... | 19.2 | 13.2 |
| 50. | Cabbage ... | 10.5 | 2.7 | 100. | Wish ..... | 20.2 | 12.7 |

Some examination was made of the classes of association in the ten longest words, but the forms of association did not seem different from those of other words; there is illustrated, however, a tendency of certain words to draw to themselves special forms of association.

Among the 2,500 observations, there were 90 of ten seconds and over in length. Terrible indeed must have been the emotional complexes stirred up in these associations if the time be directly proportional to the affective reaction. As a matter of fact, the group appears commonplace enough in everything but time, including such reactions as *fruit—apples 72, deep—ocean 51, rough—smooth 260, cabbage—leaves 65, city —town 101*. When these associations are classified, the coördination is seen to be the most common form, though there is a fair sprinkling of predicates; nothing out of the ordinary.

It is, however, significant that of these extra long responses the men gave 18 and the women 72, proportionately three times as many. The lesser freedom of the women in reacting is nowhere more strikingly evident than here. To understand these results, there is no necessity for assuming everywhere a suppression of disagreeable ideas, probable as this may be in individual cases. There need be nothing more than a perfectly commonplace difficulty in selecting a response with a sufficiently intelligent sound. The failure to do so with fair promptness calls forth a certain emotion of annoyance, which of course further increases the difficulty in responding, and this process may repeat itself indefinitely. Often by using a little 'summation,' repeating the stimulus word in a sharper tone, an entirely ordinary response is elicited with slight delay.

In six records of a series of 1,000 associations, the subjects being women nurses,[1] the following stimulus words had reaction times in every case above the median time of the subject for the series in which it occurred:

| | | |
|---|---|---|
| adorn | discourage | lame |
| alone | election | priest |
| amuse | enthusiasm | person |
| apology | escort | proverb |

[1]There are seven subjects, two reacting to only half the entire list.

| | | |
|---|---|---|
| ashore | fault | shame |
| attraction | garter | sport |
| breast | goat | starch |
| caution | greeting | strange |
| celery | imagine | surprise |
| conscience | important | swear |
| contrast | impress | trouble |
| depressing | instruction | unsteady |
| | invitation | |

There are 37 of these words, considerably more than the chance number. A scant half dozen seem to be words that one might expect to arouse intenser emotional associations in individuals generally; in a greater number, the long reaction time is related rather to the unfamiliarity of the stimulus word, while in still others no reason beyond that of chance coincidence is apparent.

Of all the words occurring, *person* seems to be that most frequently associated, either as a stimulus or as a response, with long reaction times, egocentric associations, or both. It chances that in the series it is immediately followed by the stimulus word *virtue*. Here also there are several abnormally lengthened times, the associations for both words being as follows:

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| person | man 14 | woman 26 | human 16 | woman 40 | face 19 | woman 13 |
| virtue | good 18 | reward 14 | man 8 | truth 11 | goodness 20 | virtuous 26 |

This was the only general appearance of perseverative effect.

In these experiments there were also a few instances of entire failure of the response. The stimulus words in these cases were, for the women nurses, *disgrace*, *betray*, *expect*, *information*, *cravat*, *magnet*, *diagram;* for the physician, *intend*, *never*, *interest*. Some of these words have obvious 'complexual' implication, others not.

One of these subjects had a special tendency to react with the response of *word*. As this was always accompanied by an abnormally long time, and occurred in response to such stimulus words as *useless*, *husband*, *honor*, *against*, it might have been superficially set down as a sort of 'Komplexvertreter.' The subject's own account of these reactions is simply one of a failure of associations to arise, the stimulus word retaining

paramountcy in consciousness. The response of *word* thus refers to the imagery of the stimulus word itself. The subject specifically asserted that dodging did not occur in these cases, while admitting its occurrence in other associations. A similar process may bring about failure of the response, or the repetition of the stimulus word, according to the idiosyncrasy of the subject. It is impossible to assign to the emotive element the principal rôle in all such cases, unless one prefers the short cut to consistent findings of improving upon the introspection in a way that, as Titchener has phrased it, voluntarily abandons the sphere of fact for that of fiction.

The diurnal variation in the reaction time of these 1,000 word experiments is discussed in another paper.[1] Little need be added to these remarks, and to those on the Kent-Rosanoff experiment. Perhaps it might be further emphasized that the practice effect is essentially the reduction of abnormally long times and is to be expected in those individuals, who, through personal idiosyncrasy or inferior adaption to the experiment, show such lengthened times. Owing to the fact that the individual differences in reaction time as we see them at the beginning of practice are not matters of education, it seems probable that they are not themselves matters of special practice, even though such practice tends to obliterate the differences. The determinants of long or short reaction time at the beginning of special practice are given in the original nature of the individual rather than in his training.

It has not seemed worth while to minutely analyze the greater part of the associations in these longer experiments for the purposes of this report. No change in the interpretation of the Kent-Rosanoff experiments is indicated in the analyses that have been made. These, beyond those above mentioned, are directed mainly towards throwing further light on the probable error of the free association times of the same word in the same individual, and the ever recurring question of the time as related to the emotional reaction.

First, regarding the question of the probable error.

[1] *Am. J. Psych.*, XXII., pp. 1–13.

While practice shortens the central tendencies of the times, it does not bring down their lower limit very much; the fastest four or five reactions for one series are apt to lie between 5 and 7 fifths for all subjects during all experiments. For this reason, the few shortest words in any series may be regarded as comparatively immune from any decrease in reaction time by practice. The three stimulus words having the shortest times in each individual series[1] were therefore made up into special lists, these being different of course, for each subject. Each subject was then given his own series, and there could be observed the tendency of these shortest times to remain consistently so.

When the association time of each individual stimulus word, as given in the first instance, is compared with its association time at the second presentation, the mean variations average as follows:

| Subject..... | Black | Brown | Red | Orange | Green | Blue | |
|---|---|---|---|---|---|---|---|
| M.V..... | .61 | .43 | 2.0 | 1.6 | 1.2 | .49 | fifths of a second. |

These figures indicate that where the process of responding is found to be free and unencumbered by hindrances, emotional or otherwise, the reliability of the individual measure is between one tenth and four tenths of a second for each of the different subjects. In these cases, then, the freedom of reaction seems to be a quality fairly inherent in the word, so far as the individual subject is concerned. This might not be so markedly the case with words having longer times, but it is impossible to test them directly. In order to obviate the memory factor, there must be much experimental material intervening between repetitions, and where this is the case, the longer times undergo an independent reduction by practice. The only method is to determine if the longer words maintain a similar relative position. As appears below, they show some tendency to do this; the most precise determination seems scarcely worth the labor it would involve.

From each series, three words were similarly selected whose times had fallen at the mode, and the three longest reactions

[1]Excluding the first two, and the shortest series for each subject, which were repeated separately. From the twentieth series only two words are taken.

of each series were also gathered into a separate group. The association times in these individually selected series are as follows:

| Subject. | Black. | | Brown. | | Red. | | Orange. | | Green. | | Blue. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Series. | Med. | 50 % R. | Med. | 50 % R. | Med. | 50 % R. | Med. | 50% R. | Med. | 50 % R. | Med. | 50 % R. |
| Shortest.. | 6.5 | 1.2 | 6.0 | 2.0 | 8.4 | 2.6 | 9.2 | 2.0 | 7.9 | 2.3 | 5.3 | .9 |
| Modal ... | Not given | | 7.5 | 2.4 | 11.0 | 4.4 | 10.7 | 3.4 | 9.5 | 3.0 | 7.3 | 1.5 |
| Longest .. | 9.1 | 2.7 | 12.0 | 5.0 | 16.0 | 7.6 | 11.0 | 10.0 | 15.0 | 10.0 | 8.1 | 2.8 |

As can be seen, the shortest stimulus words remain definitely shortest; in three subjects the longest approach very close to the mode, while in the others they remain a considerable distance from it. In some ways, this difference in the ability to reduce the longest times is a more significant datum than the general practice effect itself. It is most strikingly seen in Subjects ORANGE and GREEN, the former of whom has brought the longest reactions practically to the level of the modals, while in the latter a marked difference still remains. The much greater variability in the longest times is a striking, though not unnatural feature. When the associations in these series are classified there is a general tendency for the contrasts to decrease in number and for the predicates to increase, in the longest as compared with the shortest series.

To each subject was also repeated the series which had had the shortest median time in the practice experiment, the median times in each case being:

| Subject. | Black. | | Brown. | | Red. | | Orange. | | Green. | | Blue. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Series. | Med. | 50 % R. | Med. | 50 % R. | Med. | 50 % R. | Med. | 50 % R. | Med. | 50 % R. | Med. | 50 % R. |
| Shortest.. | 7.0 | 1.8 | 8.2 | 3.3 | 9.1 | 2.9 | 8.7 | 3.0 | 9.0 | 3.0 | 6.3 | 1.4 |
| Repeated . | 7.1 | 1.6 | 6.7 | 2.0 | 8.4 | 3.0 | 8.1 | 2.6 | 8.0 | 3.0 | 6.3 | 1.5 |

Except in the first and last subjects, who began nearest the limit of quickness, the time in the repeated series is noticeably shorter than in the original, illustrating the practice effect that intervenes.

In the experiments just described, as well as in a number

of additional ones,[1] the attempt was made with four of the women subjects to further observe the relations of the reaction time to the emotional response. The method was one that was developed in the laboratory for purposes connected with the 'psychogalvanic reflex.' It consisted, as elsewhere described, in causing the subject to assign the emotional response to one of four grades, which may be termed A, B, C and F, according to its greater or lesser emotional value. This procedure has undergone extended test with subjects more skilled in introspection. With the present group of subjects it naturally does not attain the reliability that it would have in the case of the more practiced introspectionists. There is every external indication that the main sources of error that a practiced introspectionist observes, enter in a greater degree into these results also. Emotional reactions of an elevated character are graded too high, those of a baser character too low, and there may be dodging in the assignment of the grades.

In certain instances, the grade assigned may indeed be very different from that which one would, on the external evidence, have estimated it to be. High grades may attach to short times associations of relatively superficial appearance, as the grade A to *hot—cold 6*, *tap—water 6*, *mountain—hill 7*, *needle—sew 7*, *hay—grass 7*, *luck—good 7;* and the grade F may attach to long-timed associations where an emotional implication lies nearer, as *flirt—naughty 24*, or *kiss—osculation 28*. It is possible to assume *ohne weiteres* that the introspective data are faulty in such cases, though it is scarcely scientific to do so. One must be guided by such introspective data as are indirectly obtainable, as well as by external acquaintance with the individual subjects.

In all, there are available for study 1,388 observations by this method, an approximately equal number with each of the four subjects. If the length of the association times is directly correlated with the intensity of the emotional response, then the central tendencies of the times attaching to each of the grades should decrease in length as the grades decrease in intensity. The figures are given in the following table:

[1]Including a translation of Jung's series beginning *Kopf, grün*, etc.

EMOTIONAL GRADES.

| Subject. | A. | | B. | | C. | | F. | |
|---|---|---|---|---|---|---|---|---|
| | Med. | 50 % R. | Med. | 50 % R. | Med. | 50 % R. | Med. | 50 % R. |
| Brown ... | 13.0 | 6.6 | 11.0 | 4.5 | 8.2 | 3.3 | 7.6 | 3.6 |
| Red ..... | 11.4 | 5.4 | 10.0 | 4.2 | 10.3 | 3.9 | 11.3 | 7.2 |
| Orange ... | 11.7 | 3.8 | 10.5 | 3.2 | 10.9 | 3.5 | 11.2 | 4.0 |
| Green .... | 10.3 | 4.6 | 9.7 | 3.7 | 9.3 | 4.2 | 10.9 | 5.0 |

The situation is seen to be complex. Only the first subject shows the progressive decrease of the times with the grades. In the first three subjects the A-grades have indeed the longest times, and in the fourth subject the next to longest; but in every subject except the first one, the times are short in the B-grades and then tend to increase to the end. In the fourth subject, the F's actually have a longer median time than the A's. It is seen that we cannot regard the emotional intensity as the sole important factor in lengthening the time, unless we are prepared to consider the grading not only unreliable but very ingeniously falsified. On the other hand, certain observations made with more practiced subjects have shown a somewhat (not greatly) closer correspondence between high grade and long time. But in view of the mutual agreement of the present results, can we attribute this entirely to more accurate introspection?

The matter shapes itself somewhat as follows: Probably no one could seriously assert that increased emotional reaction is the sole cause of lengthened association time, though it need not be questioned that it is an important contributing cause. The principal other factors are the failure to find a response that is intellectually satisfying, and a failure of predominance of any one among the associations presented. These situations, while they need not be devoid of emotional content, can scarcely have that of the associations touching on genuine emotional complexes. Other things being equal, the difficulty of intellectual satisfaction is certain to be greater in less educated than in highly educated individuals. In the present group of subjects, it therefore tends to contribute a much greater share to the lengthened association times which therefore tend to be graded low in emotional value.

The results of all the subjects except BROWN strongly indicate the presence of two such independent factors in lengthening the time; the times of the A-grades tend to form a distinct species with reference to those of the other grades. If the remaining grades, where we deal only with lesser orders of emotional intensity, were chaotic, then this interpretation would suffice, but there appears a curious tendency of the intellectual difficulty to be inversely related to the emotional reaction. No constant error of introspection seems to account for this relationship. Possibly one might here regard the affective moment as similar in action to drugs which in smaller qualities acted as stimuli, before in larger amounts they produced inhibitory effects. While beyond a certain point the intensity of the affective moment tends to volitional paralysis, up to a certain point it may easily act as a spring upon the response, and where it is conspicuously lacking, the time of the response may be lengthened through its sheer indifference.

In this connection it is worth while to mention the distribution of the different grades in the three series made up of the stimulus words which had had respectively the shortest, modal and longest reaction times. The total number of A-grades in the longest series is 42, in the modal series 40, in the shortest series 22. The number of F-grades is in the longest series 104, in the modal 79, in the shortest 75. The longest series tends to have its words graded in the extreme positions; every subject shows a drop in frequency at the C-grade, while F-grade is most numerous of all. It thus seems that there are two sorts of 'too long' reactions, one of which tends to be graded quite high, and the other quite low. This is also seen in the detailed distributions of the reaction times for the different grades; in two subjects, more reactions of 30 fifths and over are graded F than are assigned to all the other grades put together. It may be added that the number of A grades is throughout considerably greater than has been assigned by better practiced observers.

The results given and the conclusions suggested in the foregoing observations may be briefly reviewed. In respect

to sex differences in association time, the women cover a much wider range, slightly shorter times being given by individual women, as well as very much longer ones. The central tendency of the times is longer in the women. The same phenomenon is exaggerated in the variability of the times in the single individual. The women fall about equally into two distinct species, one of which is slightly more constant than the men, the other considerably less so. In respect to form of association, the women give some twice as many *Urteilsreaktionen*, and considerably fewer coördinations and supraordinations. The generally shorter times of the men are expressed in prompter reaction in the categories of supraordination, contrast, coexistence and identity, perhaps also in the more superficial predicates. The most clear cut relation of association type to time is independent of sex difference and appears in a negative relationship between the number of contrasts and the number of supraordinates. The individual with the longer time is likely to favor the supraordinates rather than the contrasts. In the Kent-Rosanoff experiments, there was evident no general tendency of the more frequent classes of associations to be shorter or longer than the less frequent ones. Further, the 90 longest associations of the experiments showed no marked peculiarities in association form. Throughout, there were few phenomena of perseveration, and indeed all the lower forms of association were relatively infrequent. The probable error of an absolutely free association time is rarely greater than 5 per cent. of the time itself.

The endeavor to systematically correlate the association times with the introspection of the emotion indicates that while associations of special emotional coloring are apt to have on the whole a longer time than the entire body of the associations, yet there are factors quite independent of the emotional response which may result in reaction times as long as or longer than those where the emotional factor is principally concerned. This is true whether the subject is practiced in introspection or not. In all cases, the variability of the times attaching to any grade of emotional re-

action is such as to render the reliability of the time as an index of the emotion, in individual observations, practically *nil*.

The free association time is exactly what its name implies, a measure of associative freedom. This involves a freedom of presentation, of selection, of expression. We obtain uniformly short association times in those individuals in whom all these processes are comparatively free. Where longer times are found, obstruction of some sort is indicated. We have traced some of the characteristics of this obstruction; of its range of variability, how under the conditions it was more marked in women than in men, especially seen in certain classes of associations, and in special stimulus words. This is as far as the unsupported association time will carry one. It remains then to become acquainted with the nature of the obstruction. Where it is found, it must not be incontinently set down to emotional factors, but the rôles of suppression, distraction, indecision, and the like, must be carefully compared and weighed by the best available means. Then only do the differences in association time assume their complete significance for the test, which is the most searching of all psychological experiments, and gives more coercively than all others, a paradigma of the subject's mental habits.

# EXPERIMENTS ON THE INHIBITION OF SENSATIONS[1]

BY EDMUND JACOBSON

§1. *Introduction.*—The inquiry as to how simultaneous sensations affect each other has an obviously fundamental importance. With apposite technique Heymans has shown that threshold sensations of color, taste, sound, pressure and light may be totally suppressed by other strong sensations respectively of the same quality. He has found also that the faint sensations caused by the ticking of a watch may be inhibited by applying a faradizing current to the fingers. On detailed observations he has based several laws, the first being that the inhibitory power of a sensation increases in proportion to its intensity, that increase being measured in terms of the stimuli to the sensations which it is able to suppress.[2]

Heymans assumes without comment that when faint sensations are inhibited, this is equivalent to a diminution of their intensity. In this he is simply accepting the word of his subject, who with his attention voluntarily directed to the sense-organ finds them weakened or absent. Were one to suppose that the faint sensation is present in the subject's consciousness, but that he is unable to get at it, one would be

[1]From the Harvard Psychological Laboratory; being a revision of an experimental chapter of a thesis on 'Inhibition' submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at Harvard University, June, 1910.

[2]For these laws he claims a far-reaching character, applying them for the explanation of the basis of sensational thresholds. In addition he calls attention to other experiments—certain ones made by himself and certain ones made by others—the results of which he interprets as showing that sensations of difference ('*Unterschiedsempfindungen*') both weak and strong, can be partially or totally suppressed by ordinary sensations of vision or the like or by other sensations of difference. This being so, he sees in it the explanation of Weber's law. Our present concern lies only with the first law. 'Untersuchungen über psychische Hemmung,' *Zeitschr. f. Psychol.*, 21, 1899, 321 f.; *ibid.*, 26, 1901, 305 f.; *ibid.*, 34, 1904, 15 f.

implicitly asserting the existence of an unconscious sensation,—which is an evident contradiction. The intensity of a sensation is precisely that in it which tends to increase if, for example, the corresponding stimulus is increased; and what Heymans finds is that he may increase the stimulus and yet the faint sensation will fail to appear in consciousness if also he sufficiently increases the stimulus to the stronger inhibiting sensation. Hence it is with justice that he assumes that the strong sensation decreases the intensity of the faint one.

Granting, however, that liminal sensations may be obliterated by other but powerful sensations either of like or of unlike quality, for instance, that one produced by a few hundred milligrams of pressure might vanish if a three hundred gram weight were applied to the skin near by—further research seems necessary in order with surety to warrant such assertions as that the three hundred gram weight would occasion an appreciable loss in a sensation far above the limen in strength, for instance, in one produced by a 20-gram weight. For it is easily conceivable that the latter might be powerful enough effectively to resist even partial inhibition; and even if it did suffer a diminution equivalent to a few hundred milligrams, we might anticipate that this loss would not be noticeable, according to the well-known data that center about Weber's law. And the whole situation is further complicated by the fact that other observers than Heymans have found that distractives may sometimes fail of their purpose and work augmentor effects instead of inhibitory. Hence it seems interesting to inquire how moderately strong sensations would be affected, if at all, by other simultaneous sensations.

First of all the problem arises as to some means for measuring the sensations in the subject's mind. At the same time, the well-known historical controversies about whether increased attention brings increased intensity to sensation make it clear that all attempts to come to definite solutions through direct appeals to the introspection of the subject would be likely to prove futile. Accordingly, it is necessary

to find some other means for measuring changes in intensity, some relatively objective scale or standard. Such an objective scale or standard is furnished in regard to any given sensation by comparing it with others subsequently or previously given and determining how many times out of ten it is judged stronger than they. If, for instance, a pressure sensation produced by 20 grams is to be used, then compare it with sensations produced by weights of 12, 15, 17, 18, 21, 23, 25, 27 and the like. In this manner one can find a rough scale in which the 20-gram weight will be judged heavier, say 10, 9, 6, 5, 4, 2, 1, 0 times out of ten than the other weights. One may now choose 2 weights $C'$ and $C$ such that the one is somewhat heavier than 20 gr. and the other somewhat lighter, and hence that the 20-gr. weight on the average is judged heavier than $C'$ from 1 to 3 times out of ten and heavier than $C$ from 6 to 9 times out of ten.[1] Then a sound may be produced simultaneously with the sensation occasioned by the application of $C'$ or $C$, and the number of times out of ten that the 20-gr. weight (given always without the sound) is now judged heavier than $C'$ and $C$ may be determined. If the 20-gr. weight is judged heavier than $C'$ more times when the sound is applied with the latter weight than when not, one would have some reason to believe that the sound diminished the sensation of $C'$. Yet this same result might be consistent with the very different assumption that the sound simply confused the judgment and rendered discrimination less exact. If 20 gr. is judged heavier than $C'$ only two times out of ten, then any disturbance of the exactitude of judgment might conceivably lead to an average increase to three or four times out of ten. But suppose that in addition to showing that the 20-gr. weight is judged heavier than $C'$ with the sound more often than without, the statistics showed also that it was judged heavier than $C$ more often when the sound occurred simultaneously with $C$ than when not: then the result would be unequivocal. For the figure for $C$ is already about eight out of ten, and there would be no reason for sup-

[1]The standard weight which we arbitrarily name here as 20 gr. may of course have other denominations. In our later discussions we shall call it $S$, leaving $C$ to stand for the lighter weight that is compared with $S$, and $C'$ for the heavier weight.

posing that disturbance of discrimination would increase this number on the average still nearer to ten. Far more would it be to be expected that poorer discrimination would tend to make the subject unable to call 20 gr. heavier than *C* so many as eight times out of ten; or else that it would increase the mean variation about eight. But if not alone the figure for *C'* but also that for *C* is increased, then there could remain no reasonable doubt that since the sound makes the heavier weight reported lighter and the lighter weight even lighter still than before, we might infer that the sound in some way diminishes the weight sensation. This method of inquiry lies at the basis of Series 1.

## Series 1.

### *Inhibition of Pressure Sensations by Sound Sensations.*

§2. *Apparatus.*—Sound sensations of moderately strong intensity were produced by means of an electrical buzzer always placed 30 inches from the left ear of the subject, whose head was secured by means of a head-rest. The sound consisted essentially of loud clicks in so quick succession as almost to be fused. The sound varied somewhat from day to day, particularly because of variations in the current, but it was sufficiently satisfactory for the present purpose.

Pressure sensations were produced by means of the instrument used by Kobylecki and originally invented in a simpler form by Stratton. The former has given a full description of it in *Wundt's Psychologische Studien*, Vol. 1, and it will suffice here to give a simple explanatory sketch of the machine as it was adapted for the purposes of Series 1 (cf. Fig. 1). The subject sat facing the machine with elbow supported, his right hand placed palm downward on the rest indicated under *T*. As will be seen, only two levers, *AB* and *DC*, are used, and these are evenly balanced and move freely. At the point *A* there projects downward a rounded stick of light wood. The lower end of *T* did not come in contact with the hand, but rested on a cork disc about 15 mm. in diameter, the position of which was usually somewhere between the knuckles of the middle and fore-finger. Other locations,

also, were used at times. It will be apparent from the sketch that a weight placed on the pan *R* will cause *T* to move downwards and thus communicate pressure to the cork disc on the hand. It is advisable, however, to have the lower end of *T* always in contact with the cork disc, else the application of the weight to the pan *R* will cause *T* to fall suddenly and thus give a sensation of impact instead of mere pressure. The screw *K* can be used for this purpose but it works very

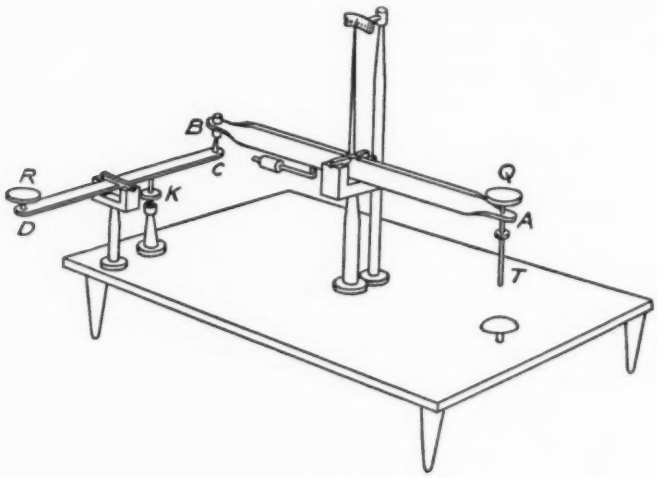



FIG. 1.

badly in that the subject's hand often shifts slightly or changes in volume, when the lower end of *T* may easily come to lose contact with the cork, or on the other hand, may come to press tightly on it. Hence *K* was not used at all, but instead a 3-gr. weight was allowed always to remain on the pan *R*, and this kept *T* in continual light contact with the cork in a very satisfactory manner. The weights were applied to the pan *R* by putting them on a piece of stiff but soft blotting paper and lowering them gently to the pan, a thing which required much practice on the part of the operator.

§3. *Procedure.*—The operator gave a signal by saying 'One!' in a low voice and then placed the weight on the pan *R*, removing it after four seconds. After an interval also of four seconds, he gave the signal 'Nine!' and placed the second

weight on the pan, removing it again after four seconds. The subject pronounced judgment with respect to the second weight saying that it was heavier, lighter or the same as the first.

The session with each subject occurred once per week, the actual work lasting usually about forty or forty-five minutes. The first part of the period usually was devoted to practice, but the number of judgments recorded in a successful session averaged between forty and eighty. During the session rest-periods of a few minutes each were given,—sometimes one or three, but more often two. Nevertheless there were times when the subject complained of cramp of the arm, which came from keeping it in a constant position. While fatigue of the sense-organ had to be taken into account and as much as possible avoided, yet it seemed to present no grave difficulty. The position of the cork disc on the hand was changed now and then at the instance either of the operator or of the subject, the latter being requested to move the disc when it seemed necessary.[1] When the attention was good, the judgments proved on the whole as reliable toward the end of the hour as at the beginning. We may add the detail that there was daylight illumination, but that the subject sat with closed eyes. Unfortunately the room was not sound-proof, and hence it was frequently necessary to wait until some outside noise should cease, and even to repeat a set of weights which had been judged. A repetition was also made whenever in the judgment of the subject or operator the weights were put on the pan in improper manner. Repetitions were specially recorded in the protocol except in the first part of Series I. A special sign was used to mark whenever the subject found that owing to poor attention or the like he was in doubt and unable to judge. Every effect was made to have him properly distinguish between cases in which he ought to judge 'the same' and those in which he ought to report himself really unable to judge. In all likelihood, however, many cases were pronounced 'doubtful' which should have been called 'the same' and *vice versa*, for the distinction is often very difficult

[1]Rests and changes of the position of the cork were recorded in the protocol.

indeed. It scarcely need be added that the operator continually had in mind the danger of unconsciously communicating to the subject anything that might influence his judgments. Hence care was taken in uttering the signals and in other similar respects. The personal equation of the operator in this series certainly introduces a large error, but in a subsequent series where sounds mechanically produced were judged instead of weights, the general character of the results obtained were the same as those of this series, and hence that error may to some extent be disregarded. So far as possible, the ultimate aims of the experiment were kept from the knowledge of the subject.

It will be recalled that the method of procedure is to select a convenient weight *S* which produces a pressure sensation of moderate strength and then to find two other weights *C* and *C′*, one lighter and the other heavier, as described on page 26. Because of the well-known tendency to overestimate the strength of the second of two sensations, it is necessary to distinguish between the orders *S–C* and *C–S*. For if *S* is judged heavier than *C* a certain number of times out of ten when given in the former order, one may expect it to be judged heavier a different number of times out of ten in the latter order.[1] We have, then, the couplets *S–C*, *C–S*, *S–C′* and *C′–S*, which we shall designate respectively by the letters A, B, E and F. Our purpose is to find the number of times out of ten that *S* is judged heavier than *C* or *C′*, in each of these orders, and then to produce the sound simultaneously with *C* and *C′* (but not with *S*) and to find how many times *S* is then judged heavier. Accordingly, if we let the symbol */n* stand for the occurrence of the sound, we have also the couplets *S–C/n*, *C/n–S*, *S–C′/n*, and *C′/n–S*, which we shall designate respectively by the letters C, D, G and H. Because of personal peculiarities of sensibilitity and discrimination power, different sets of weights were used for each subject, and this is indicated in Table I. For example, it will be seen that for subject 1 $S = 14$, $C = 10$, and $C' = 18$, while for subject 2 $S = 20$, $C = 15$, and $C' = 25$.

[1]Usually a larger number of times in the order *C–S*, according to my own experience.

TABLE I.[1]

| | Subject 1. | | Subject 2. | | Subject 3. | | Subject 4. | | Subject 5. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). |
| A | 14 | 10 | 20 | 15 | 22 | 18 | 24 | 18 | 20 | 10 |
| B | 10 | 14 | 15 | 20 | 18 | 22 | 18 | 24 | 10 | 20 |
| C | 14 | 10/*n* | 20 | 15/*n* | 22 | 18/*n* | 24 | 18/*n* | 20 | 10/*n* |
| D | 10/*n* | 14 | 15/*n* | 20 | 18/*n* | 22 | 18/*n* | 24 | 10/*n* | 20 |
| E | 14 | 18 | 20 | 25 | 22 | 26 | 24 | 30 | 20 | 30 |
| F | 18 | 14 | 25 | 20 | 26 | 22 | 30 | 24 | 30 | 20 |
| G | 14 | 18/*n* | 20 | 25/*n* | 22 | 26/*n* | 24 | 30/*n* | 20 | 30/*n* |
| H | 18/*n* | 14 | 25/*n* | 20 | 26/*n* | 22 | 30/*n* | 24 | 30/*n* | 20 |

Since the subject is instructed to pass judgment in terms of the second weight given it will be apparent that if we count the number of judgments 'lighter' in the cases of A, C, E and G, but the number of judgments 'heavier' in the cases of B, D, F and H, we shall in each case have a figure representing the number of times that *S* is judged heavier than its mate. It is obvious also that A is to be compared with C, B with D, E with G, and F with H; or similarly, (A+B) with (C+D), and (E+F) with (G+H); or again, (A+B+E+F) with (C+D+G+H). Following a practice that is general in experiments with weight sensations, we reckon the judgment 'same' as equivalent to one half 'heavier' or one half 'lighter.' In other words 'same' means midway between 'heavier' and 'lighter.' Series 1 consisted of four parts in each of which ten judgments were passed on each of the couplets A, B, C, D, E, F, G, H given in haphazard order. Hence altogether there were 4×10 judgments passed on each of eight couplets, making a total of 320 judgments for each subject.

§4. *Results.*—The results of Series 1 are presented in Table II. As the explanatory note under the table indicates, (which see) each figure in roman type is to be compared with the corresponding one in italic. Even a careless glance will show that the italic figures are in a good majority of the cases larger than those in roman. Out of the eighty cases in this table where one may compare the figure for a single letter in roman (A or B or E or F) with the corresponding one in italic, an increase occurs

[1]The sign (/*n*) signifies the occurrence of the sound.

53 times, an equality 14 times, and a decrease only 13 times. The increase is made especially clear in the sums in the table. It can also be shown in the averages;—for instance, the total averages for all subjects reckoned together are as follows: A = 7.5, B = 8.2, C = 8., D = 8.7, E = 1.7, F = 3.5, G = 2.8, H = 4.6.

TABLE II.

| | | Subject 1. | | Subject 2. | | Subject 3. | | Subject 4. | | Subject 5. | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Series 1–A. | | | | | | | | | | | |
| A | *C* | 9 | *10* | 4½ | *8* | 8½ | *8* | 7 | *7* | 9 | *9* |
| B | *D* | 9 | *9* | 9 | *9* | 8 | *8* | 7 | *7½* | 9 | *10* |
| A+B | *C+D* | 18 | *19* | 13½ | *17* | 16½ | *16* | 14 | *14½* | 18 | *19* |
| E | *G* | 3 | *5* | 0 | *1½* | 2 | *5* | 6 | *3½* | 0 | *2½* |
| F | *H* | 3 | *4* | 5½ | *4½* | 6 | *8* | 1½ | *3* | 3 | *4½* |
| E+F | *G+H* | 6 | *9* | 5½ | *6* | 8 | *13* | 7½ | *6½* | 3 | *7* |
| A+B+E+F | *C+D+G+H* | 24 | *28* | 19 | *23* | 24½ | *29* | 21½ | *21* | 21 | *26* |
| Series 1–B. | | | | | | | | | | | |
| A | *C* | 8 | *9* | 3½ | *5* | 5 | *7* | 8 | *8* | 9½ | *8* |
| B | *D* | 8 | *8½* | 10 | *10* | 9 | *6* | 7½ | *10* | 10 | *9½* |
| A+B | *C+D* | 16 | *17½* | 13½ | *15* | 14 | *13* | 15½ | *18* | 19½ | *17½* |
| E | *G* | 1 | *2* | 1 | *6½* | 4 | *2* | ½ | *2* | 2 | *1* |
| F | *H* | 4½ | *5* | 5 | *7½* | 4 | *9* | 4 | *4* | 2 | *2* |
| E+F | *G+H* | 5½ | *7* | 6 | *14* | 8 | *11* | 4½ | *6* | 4 | *3* |
| A+B+E+F | *C+D+G+H* | 21½ | *24½* | 19½ | *29* | 22 | *24* | 20 | *24* | 23½ | *20½* |
| Series 1–C. | | | | | | | | | | | |
| A | *C* | 9½ | *7½* | 8 | *9½* | 7 | *8* | 6½ | *8* | 7½ | *9½* |
| B | *D* | 7½ | *9* | 9 | *7* | 8 | *9* | 10 | *10* | 10 | *10* |
| A+B | *C+D* | 17 | *16½* | 17 | *16½* | 15 | *17* | 16½ | *18* | 17½ | *19½* |
| E | *G* | 1 | *5* | 4 | *4* | 2½ | *4* | 0 | ½ | ½ | *1½* |
| F | *H* | 2½ | *3* | 4½ | *5½* | 3 | *4* | 4 | *2* | 2½ | *5½* |
| E+F | *G+H* | 3½ | *8* | 8½ | *9½* | 5½ | *8* | 4 | *2½* | 3 | *7* |
| A+B+E+F | *C+D+G+H* | 20½ | *24½* | 25½ | *26* | 20½ | *25* | 20½ | *20½* | 20½ | *26½* |
| Series 1–D. | | | | | | | | | | | |
| A | *C* | 8½ | *9* | 8 | *8½* | 7 | *5* | 7 | *8½* | 8 | *8* |
| B | *D* | 5 | *8* | 8 | *5½* | 4 | *9* | 8 | *9* | 8½ | *10* |
| A+B | *C+D* | 13½ | *17* | 16 | *14* | 11 | *14* | 15 | *17½* | 16½ | *18* |
| E | *G* | 4 | *2½* | 0 | *3½* | 1 | *2* | ½ | *1½* | 1 | *1* |
| F | *H* | 2½ | *4* | 2 | *2* | 4 | *5* | 3 | *5* | 3½ | *4½* |
| E+F | *G+H* | 6½ | *6½* | 2 | *5½* | 5 | *7* | 3½ | *6½* | 4½ | *5½* |
| A+B+E+F | *C+D+G+H* | 20 | *23½* | 18 | *19½* | 16 | *21* | 18½ | *24* | 21 | *23½* |
| Total sum | *Total sum* | 86 | *100½* | 82 | *97½* | 83 | *99* | 80½ | *89½* | 86 | *96½* |

Figures opposite single letters in roman type (A or B or E or F) in each case represent the number of times a given weight (*S*) was judged heavier than another (*C* or *C'*); while in the space to the right of each such figure in roman is a corresponding figure in italic which represents the number of times that same weight (*S*) was judged heavier than the same other one (*C* or *C'*) when the sound was made simultaneously with the latter.

*In other words, the number of times that a given weight, S, was judged heavier than another was on the whole increased when a sound was made simultaneously with the application of that other.* That the influence of the sound was not essentially to confuse the judgment was learned firstly from the reports of the subjects who soon became accustomed to it, and secondly, it is shown by the figures themselves. For it is generally true, as we have seen, that not alone are the figures for E and F which average respectively 1.7 and 3.5 materially increased in G and H, but what is especially significant, so also are the figures for A and B increased in C and D, despite the fact that these average above 7. Hence the figures seem conclusively to indicate that the sound diminishes the weight sensation.[1] The subjects thought themselves to some extent able to verify the inhibiting effects of the sound when (after the year's work was over) they were told of it and special tests were repeated. Previously, however, they had not been able to guess the effect of the sound for themselves and similarly they were not able to notice that when sounds occurred in the hall or out-doors their judgments were affected thereby. *This indicates the interesting fact that one may be distracted and yet be unaware of it.*

That the diminution is not due to the memory but lies rather in the sensation process itself is shown by a simple calculation: In the cases of D and H, the sound occurs with the first weight which is remembered and thus compared

[1]Upon being questioned as to whether the sound made the concomitant pressure less 'clear,' three of the subjects replied that it did in some cases, but often or usually it did not seem to. In this connection it must be remembered that the sound was repeated again and again in the course of the experiment. Two of the subjects thought it possible or likely that the sound had a characteristic effect to reduce the clearness of the pressure sensation. The subjects were not in agreement in their opinions as to whether a reduction of clearness in the pressure sensation would lead them to judge the corresponding weight lighter than otherwise, some believing it might, others that it would not. However, if confidence in judgment is a function of the clearness of sensations judged, then it is pertinent to mention that for the most part, though not always, the subjects said that they had about the same degree of confidence in the truth of their judgements when the sound came as when it did not. Excepting, however, that the operator frequently noted not a decrease but rather a very marked increase in confidence as expressed in the tone of voice with which a given weight was judged heavier than a lighter one when the sound occurred with the latter.

with the second. But the total average increase from B to D is—as calculated from the figures just given—.5 while that from F to H is 1.1. Now in the cases of C and G the sound occurs with the second weight and here the subject often judges while the latter is still on his hand and hence memory in no way enters in as a significant factor. But the increases from A to C and from E to G are respectively .5 and 1.1, thus showing that the diminution of the sensation is not due to fading of the memory image.

The subjects remarked that they often judged heaviness by the distance which the rod *T* seemed to push down into the flesh. To the extent to which they did this they were estimating weights perhaps not simply by strengths of sensation but rather by qualitative characteristics of the sensations. But in the latter event one would reasonably expect that the sound would have no influence on their judgments of heaviness, excepting, perhaps, to render them less confident and constant. If, therefore, one finds—as we actually have—that it is in general true that a sound concomitant with a pressure tends to make the corresponding weight judged less heavy than otherwise, it seems warranted to conclude that *the sound diminishes the strength or intensity of the weight sensation.*[1] To be sure additional evidence is very welcome, and this is furnished by analogy in Series 2.

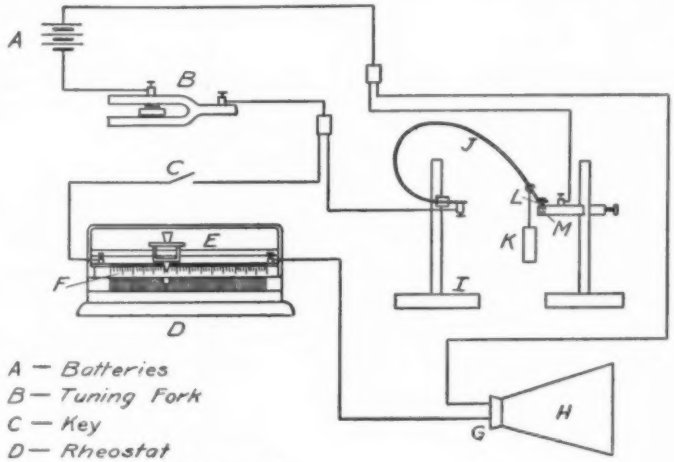
## Series 2.

### *Inhibition of Sound Sensations by Pressure Sensations.*

§5. *Apparatus.*—In this series it was undertaken to discover whether sound sensations of moderate strength can be inhibited by pressure sensations. The gravity phonometer is generally used to make sound sensations of various intensities, but it is very inferior to the tuning-fork which may

[1]The strength of the conscious sensation—the subjective intensity—is meant here since the sound of course does not interfere with the objective stimulus. It is convenient to give the name of objective intensities to correspond proportionately with the objective stimuli, though these are essentially fictional, and not real entities, since the real intensity of a conscious sensation—as we seek here to demonstrate—depends not solely on the quantity of the objective stimulus, but decidedly upon subjective conditions as well.

be arranged as shown in the accompanying sketch (Fig. 2). An electrical tuning-fork of 256 vibrations was placed in an adjoining room and connected in single series with a key *C*, a rheostat *D* and a telephone receiver *G*, to which was attached a megaphone *H*. To the rheostat ('*G–R*' rheostat, with Gebr. Ruhstrat composition-wire resistance) was attached a millimeter scale along which the slider *E* moved, thus getting variation of the current that passed through the tele-




A — Batteries
B — Tuning Fork
C — Key
D — Rheostat
E — Slider of Rheostat.
F — Scale " " "
G — Telephone Receiver
H — Megaphone
I — Porter's Interruptor
J — Vibrating Spring
K — Weight
L — Contact Point
M — Mercury

FIG. 2.

phone receiver and hence getting corresponding variation of the loudness of the sound.

If the apparatus consisted merely of the fork, rheostat, megaphone, key and connecting wires, one could not produce sounds of short duration at will, for it would be necessary to go into the other room to start up the tuning-fork each time a sound were desired. Now it is necessary so to arrange the apparatus that the tuning-fork is kept going all the time. To this end the leads from the tuning-fork were also connected up with wires leading through the apparatus marked *I*.

This instrument is known as 'Porter's Interrupter' and consists of a spring *J* which is bent over and held down by a weight *K*. The spring can be arranged with various vibrating lengths, but in the present experiment it was so arranged that it made a vibration in half a second. The contact point *L* was arranged so that it just touched the surface of the mercury *M*. Now when *K* was pulled down a short distance by the hand of the operator and then released, *L* would rise out of the mercury and thus break the circuit for one half second. With the key *C* closed and *L* in contact with *M* practically all the current would always pass through the wires that lead through *J*, because here the resistance was very much less than through *D*. But upon breaking contact with *M*, the current would be forced to go through *D* and *G*, thus making a sound one half second in duration, the relative intensity of which could be fixed at will by moving the slider *E*.[1]

For the pressure sensations the apparatus was used as shown in Fig. 3. The hand of the subject was placed as before with the cork disc upon it under *T*. A constant weight of 10 gr. was placed under the pan *R*, thus keeping *T* in contact with the cork. Throughout Series 2, the pans *R* and *S* each bore a weight of 300 gr. The one, however, causes pressure down on the point *B* and the other up, so that each neutralizes the other so far as any motion of the lever *A–B* is concerned. In order to apply a sudden pressure of 300 gr. to the cork on the subject's hand, the operator suddenly pressed down on the lever *E–F* at the point *F*. Since the pressure sensation should be simultaneous with the sound it was very important for the operator to press down on *F* with the left hand simultaneously with the commencement of the sound. The pressure on the hand of course outlasted the sound, for it was not possible under the conditions to make its duration only one half second. The essential thing, however, is to have the pressure commence with the sound and last at least as long.

[1]Of course what is actually secured is a satisfactory approximation to one half second rather than the precise attainment. It makes for accuracy to pull down *K* an equal distance each time.

§6. *Procedure.*—On the whole this was like that of Series 1, except that the signals 'One!' and 'Nine!' were omitted, the only signal used being the word 'ready' followed by the sound a little thereafter. The second sound occurred six seconds after the first. The subject was asked to judge intensities (*i. e.*, loudness) of sound and to reply 'fainter' or 'louder,' or 'same' with reference to the second sound. Slight qualitative differences in the sound did, it is true, enter in as an error, and the subject was warned against confusing these with quantitative differences and against associating qualitative peculiarities with any given intensity, so as to try to identify the latter thereby. For while a somewhat

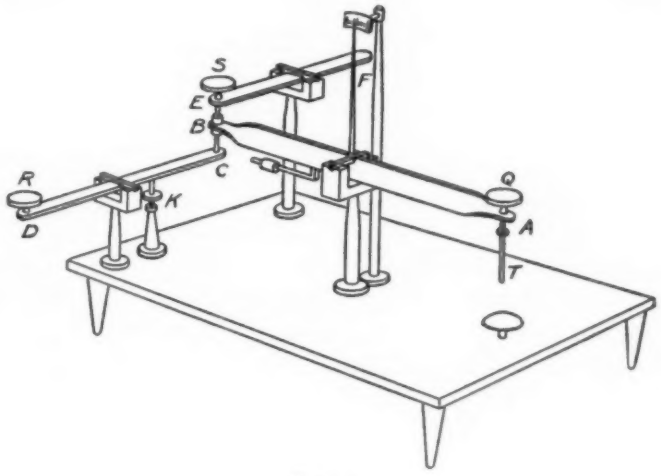



FIG. 3.

raucous tone, for instance, often characterized the stronger sounds, so also did it sometimes characterize the faint sounds, and hence it was explained to the subjects that they must not be misled thereby. Therefore, while the judgments doubtless were frequently influenced by qualitative differences, we are safe in believing that what was compared on the whole was essentially intensities of sounds.

At first the operator was in doubt as to what duration the sound should have. Various durations such as 3, 2 and 1 second were tried during the period when he was trying to

learn what was the requisite technique. Usually the practice results with these durations showed an inhibiting effect of the pressure on the sound, but often they did not. In order to obtain exact results the operator concluded that the proper duration of the sound should be one half second, firstly because this would largely preclude the possibility that the subject's attention should adapt itself to the pressure while the sound was still in operation, and secondly because it is possible that the pressure sensation quickly loses in strength.

After the subjects had been given considerable practice the operator determined what strengths of sound should be given to each of them. Because of personal differences in discrimination power, the same differences of sound could not be used for all subjects alike. Table III. indicates the relative strengths of sounds given together with the order in which

TABLE III.

| | Subject 3. | | Subject 4. | | Subject 9. | | Subject 5. | |
|---|---|---|---|---|---|---|---|---|
| | First Sound Given. | Second Sound Given. | First Sound Given. | Second Sound Given. | First Sound Given. | Second Sound Given. | First Sound Given. | Second Sound Given. |
| A | 6. | 7. | 6.5 | 8. | 6.2 | 7.4 | 8.7 | 9.4 |
| B | 7. | 6. | 8. | 6.5 | 7.4 | 6.2 | 9.4 | 8.7 |
| C | 6. | 7./300 | 6.5 | 8./300 | 6.2 | 7.4/300 | 8.7 | 9.4/300 |
| D | 7./300 | 6. | 8./300 | 6.5 | 7.4/300 | 6.2 | 9.4/300 | 8.7 |
| E | 6. | 5. | 6.5 | 5. | 6.2 | 5. | 8.7 | 8. |
| F | 5. | 6. | 5. | 6.5 | 5. | 6.2 | 8. | 8.7 |
| G | 6. | 5./300 | 6.5 | 5./300 | 6.2 | 5./300 | 8.7 | 8./300 |
| H | 5./300 | 6. | 5./300 | 6.5 | 5./300 | 6.2 | 8./300 | 8.7 |

they occurred. The numbers stand for sounds varying with the positions of the slider *E* of the rhesotat and have no precise significance except that the smaller the number, the louder is the corresponding sound. The sign /300 indicates the application of a pressure of 300 gr. We shall once more use the symbol *S* for the standard sound (6. for Subj. 3), while *C* represents the fainter, and *C'* the louder sound compared with it.

§7. *Results.*—It will be seen that the results shown in Table IV. are of analogous nature to those of Series 1. The italic figures are larger than the corresponding roman ones in 29 out of 32 cases, and this means that a given sound (*S*) was

judged louder than another sound (*C* or *C'*) when a simultaneous pressure occurred with the latter than when it did not. We are permitted to infer that *the conscious intensity of sound sensations may be reduced by concomitant pressure sensations*, a thing which harmonizes with and helps to confirm the results of Series 1.

These results are in agreement, moreover, with those of an experiment performed by Bentley, who used various sounds produced by means of the gravity phonometer with odors as distractives. He found that the odor caused the sound with which it was given to be 'underestimated,' and concluded that distraction may reduce the intensity of sensation.[1] Heymans, also, asserts that sensations above the limen may suffer a partial loss of intensity, saying that in his experiments when a given stimulus was able to produce a faint sensation despite the presence of another stronger sensation, then upon the removal of the latter the subject reported a perceptible increase of intensity. Other evidence, he claims, lies in the well known phenomena of intensity-contrast, and he undertakes special experiments in light and brightness contrast to show that this is so.[2] Finally there are the experiments of McDougall which show inhibition in the rôle of a reduction of psychic intensity. On page 185 of his 'Inhibitory Processes within the Nervous System' (*Brain*, 26, 1903) he speaks of 'sensation-intensity' as being 'subjectively diminished.' Again in a discussion of Fechner's paradox in connection with inhibition he speaks of 'loss of brightness,' which is of course loss of visual intensity.[3] And when in his series of articles on vision in *Mind*, 26, N. S., 10, he continually speaks of the 'fading' of images he is apparently speaking of loss of intensity. Hence there seems in general no room for doubt but that the subjective intensity of sensations may be reduced by the presence of other sensations.

[1]Bentley, PSYCHOL. BULLETIN, IV., 1907, 212 f. Referred to by Titchener *Psychol. of Feeling and Attention.*

[2]Heymans, *ibid.*, 41, 1906, 28 f. and 89 f.

[3]*Brit. Jour. of Psy.*, 1, 1904–05, 114–115.

TABLE IV.

| | | Subject 3. | | Subject 4. | | Subject 9. | | Subject 5. | |
|---|---|---|---|---|---|---|---|---|---|
| Series 2–A. | | | | | | | | | |
| A | *C* | 5½ | *8½* | 4½ | *6½* | 4 | *10* | 6½ | *7½* |
| B | *D* | 8 | *10* | 9½ | *9½* | 9 | *10* | 7½ | *9½* |
| A+B | *C+D* | 13½ | *18½* | 14 | *16* | 13 | *20* | 14 | *17* |
| E | *G* | ½ | *1* | 1 | *2* | 0 | *5* | 0 | *4* |
| F | *H* | 4½ | *7* | 4 | *7½* | 5 | *9* | 5 | *6* |
| E+F | *G+H* | 5 | *8* | 5 | *9½* | 5 | *14* | 5 | *10* |
| A+B+E+F | *C+D+G+H* | 18½ | *26½* | 19 | *25½* | 18 | *34* | 19 | *27* |
| Series 2–B. | | | | | | | | | |
| A | *C* | 7 | *8* | 0 | ½ | 2 | *8* | 5½ | *9½* |
| B | *D* | 9 | *10* | 2½ | *7½* | 8 | *10* | 7 | *9* |
| A+B | *C+D* | 16 | *18* | 2½ | *8* | 10 | *18* | 12½ | *18½* |
| E | *G* | ½ | *0* | 7 | *9½* | 1 | *3* | 3 | *6* |
| F | *H* | 4 | *4* | 9½ | *10* | 8 | *9* | 2½ | *7½* |
| E+F | *G+H* | 4½ | *4* | 16½ | *19½* | 9 | *12* | 5½ | *13½* |
| A+B+E+F | *C+D+G+H* | 20½ | *22* | 19 | *27½* | 19 | *30* | 18 | *32* |
| Total sum | *Total sum* | 39 | *48½* | 38 | *53* | 37 | *64* | 37 | *59* |

Figures opposite single letters in roman type (A or B or E or F) in each case represent the number of times a given sound (*S*) was judged louder than another one (*C* or *C'*); while in the space to the right of each such figure in roman is a corresponding figure in italic which indicates the number of times *S* was judged louder than *C* or *C'* when a pressure of 300 gr. was simultaneously applied to the right hand.

## SERIES 3.

### *The Inhibitory Effect on Sound Sensations of Pressure Sensations of Different Intensities.*

§8. *Apparatus.*—For sounds, same as in Series 2. For pressure sensations likewise, except that here weghts of 100 gr., 300 gr., and 500 gr. were used.

§9. *Procedure and Results.*—Only two subjects were used in this series (subjects 5 and 2). The subjoined table (V.) is practically self-explanatory. It shows, for instance, in the column headed 'Series 3–A' that the sound 5.7 was judged louder than 5 one time out of ten when there was no simultaneous pressure, one time out of ten when a pressure of 100 gr. occurred with 5, also one time with a pressure of 300 gr., and four times with a pressure of 500 gr. Thus the reader will be able easily to interpret the table. The observations are of course too few in number to permit quantitative formulations, but at least they suggest it as probable that *the greater*

*the stimulus the stronger tends to be the inhibitory power of the corresponding sensation*, a thing which one would naturaly presume to be true.

## Series 4.

### *The Inhibitory Effect of Inattention on Pressure Sensations.*

§10. *Introduction.*—Hereupon a very interesting problem develops itself. It is apparently a very fundamental relation of mind if when two sensations are simultaneously presented, the one may inhibit the other. Now what part, if any, does attention play in all this? May we assume, as would be quite natural, that what the inhibitor does is substantially to dis-

Table V.[1]

| Subject. | | First Sound Given. | Second Sound Given. | Number of Times Out of Ten First Sound Judged Louder. Series 3-A. | Series 3-B. | Series 3-C. | Series 3-D. | Total. |
|---|---|---|---|---|---|---|---|---|
| 5 | A | 5·7 | 5. | 1 | 1 | 2½ | | 4½ |
| | B | 5·7 | 5./100 | 1 | 3 | 2 | | 6 |
| | C | 5·7 | 5./300 | 1 | 1 | 1 | | 3 |
| | D | 5·7 | 5./500 | 4 | 3½ | 2½ | | 10 |
| 2 | A | 6. | 5. | 1½ | 1½ | 0 | 1 | 4 |
| | B | 6. | 5./100 | 1½ | 1 | 1 | 2 | 5½ |
| | C | 6. | 5./300 | 3½ | 2 | 1 | ½ | 7 |
| | D | 6. | 5./500 | 6 | 2 | 2 | 2 | 12 |
| | | First Sound Given. | Second Sound Given. | Number of Times Out of Ten Second Sound Judged Louder. Series 3-A. | Series 3-B. | Series 3-C. | Series 3-D. | Total. |
| 5 | E | 5·5 | 6. | 3½ | 5 | 5 | | 13½ |
| | F | 5·5/100 | 6. | 8 | 7½ | 3 | | 18½ |
| | G | 5·5/300 | 6. | 5 | 7 | 6 | | 18 |
| | H | 5·5/500 | 6. | 8 | 8½ | 5½ | | 22 |
| 2 | E | 5·3 | 6. | 2½ | 5 | 5 | 4½ | 17 |
| | F | 5·3/100 | 6. | 7½ | 6 | 4½ | 3½ | 21½ |
| | G | 5·3/300 | 6. | 8 | 6½ | 7½ | 3 | 25 |
| | H | 5·3/500 | 6. | 8 | 6½ | 9 | 8½ | 32 |

tract the attention from the inhibited? It seems inviting not to asume this, but rather to institute a new inquiry whether it is so. Thus we have encountered the old and difficult question whether increased attention brings increased intensity to sensation. But in certain respects we have unusual facili-

[1]Refer to p. 40.

ties for its solution, in as much as we are already supplied by the foregoing series with very efficient distractives together with the correct means to use them. The way is, namely, so to arrange the conditions that the distractive begins with the sensation distracted from. The importance of this—which I believe is not generally understood—deserves special emphasis. For if the the inception of the distractive is before or long after that of the sensation to be distracted from, it is likely to lose much of its inhibitory power, and it is even possible—so far as we at present know—that it may under certain conditions augment instead of inhibit. It is imperative, moreover, that the sensation distracted from be not of too long duration. Else once more the distractive—by virtue of a principle which we shall later develop—may lose its initial inhibitory influence and eventually indirectly produce an augmentation. Doubtless here lies the source of the conflicting reports well known to the literature about distractives.

Hence we need to make the time-periods short as well as simultaneous. One more special condition needs to be fulfilled if we are to develop a precise means of treatment of the problem of voluntary attention and intensity. This condition requires that contrary to the usual methods, the distractive must not be absent in one set of observations and present in the other set which is to be compared therewith, but is rather to be present in both sets, the important point of difference being that in one set the attention is voluntarily put on the distractive, while in the other it is not. Thus we will be permitted to abstract from the influence of the distractive as such, and may learn what are the differences produced by the voluntary attention alone. This passage will become clearer after we have presented and explained our tables.

§11. *Apparatus and Procedure.*—The apparatus was the same as that of Series 1, pressure sensations of four seconds in duration being produced and judged. The principle of procedure was to select a convenient weight *S*, together with a series of five other weights and to make two classes of observations with the purpose of comparing their results: first, to find out how many times out of ten the *S* weight—

always given first—would be judged heavier than each of these weights if the sound always occurred with the application of the latter while the attention was allowed naturally to fall on the pressures which were to be compared; and second, to find out how many times *S* would be judged heavier than each of these weights under the same conditions as before except that the attention were kept on the sound during its occurrence, but after its cessation transferred to the right hand, so that the sensation from the *S* weight was always received with normal or undistracted attention. In Table VI., which shows what weights were used, the capital letters A, B, C, D, E, serve to designate the couplets of weights compared with the attention resting naturally on the weight

TABLE VI.

| | Subject 6. | | Subject 7. | | Subject 8. | | Subject 9. | |
|---|---|---|---|---|---|---|---|---|
| | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). |
| A | 18/*n* | 20 | 12/*n* | 15 | 10/*n* | 12 | 10/*n* | 12 |
| B | 20/*n* | 20 | 15/*n* | 15 | 12/*n* | 12 | 13/*n* | 12 |
| C | 23/*n* | 20 | 18/*n* | 15 | 13/*n* | 12 | 16/*n* | 12 |
| D | 25/*n* | 20 | 21/*n* | 15 | 15/*n* | 12 | 19/*n* | 12 |
| E | 28/*n* | 20 | 24/*n* | 15 | 18/*n* | 12 | 22/*n* | 12 |
| a | 18/*n* | 20 | 12/*n* | 15 | 10/*n* | 12 | 10/*n* | 12 |
| b | 20/*n* | 20 | 15/*n* | 15 | 12/*n* | 12 | 13/*n* | 12 |
| c | 23/*n* | 20 | 18/*n* | 15 | 13/*n* | 12 | 16/*n* | 12 |
| d | 25/*n* | 20 | 21/*n* | 15 | 15/*n* | 12 | 19/*n* | 12 |
| e | 28/*n* | 20 | 24/*n* | 15 | 18/*n* | 12 | 22/*n* | 12 |

sensations, while the small letters, a, b, c, d, e, designate the couplets in which the attention was voluntarily concentrated on the sound. The sign /*n* stands for the sound.

Of course practice was necessary before the subjects learned to concentrate on the sound (*i. e.*, in the cases of the small letters). They were told that in case such concentration made them unable to judge with confidence, they should mention the fact and withhold their reports if necessary. Their experience was, however, that when the concentration was directed to the sound and the concomitant pressure therefore less strongly attended, there nevertheless remained a good memory image of that pressure which enabled them to judge it in a very satisfactory way.

ties for its solution, in as much as we are already supplied by the foregoing series with very efficient distractives together with the correct means to use them. The way is, namely, so to arrange the conditions that the distractive begins with the sensation distracted from. The importance of this—which I believe is not generally understood—deserves special emphasis. For if the the inception of the distractive is before or long after that of the sensation to be distracted from, it is likely to lose much of its inhibitory power, and it is even possible—so far as we at present know—that it may under certain conditions augment instead of inhibit. It is imperative, moreover, that the sensation distracted from be not of too long duration. Else once more the distractive—by virtue of a principle which we shall later develop—may lose its initial inhibitory influence and eventually indirectly produce an augmentation. Doubtless here lies the source of the conflicting reports well known to the literature about distractives.

Hence we need to make the time-periods short as well as simultaneous. One more special condition needs to be fulfilled if we are to develop a precise means of treatment of the problem of voluntary attention and intensity. This condition requires that contrary to the usual methods, the distractive must not be absent in one set of observations and present in the other set which is to be compared therewith, but is rather to be present in both sets, the important point of difference being that in one set the attention is voluntarily put on the distractive, while in the other it is not. Thus we will be permitted to abstract from the influence of the distractive as such, and may learn what are the differences produced by the voluntary attention alone. This passage will become clearer after we have presented and explained our tables.

§11. *Apparatus and Procedure.*—The apparatus was the same as that of Series 1, pressure sensations of four seconds in duration being produced and judged. The principle of procedure was to select a convenient weight *S*, together with a series of five other weights and to make two classes of observations with the purpose of comparing their results: first, to find out how many times out of ten the *S* weight—

always given first—would be judged heavier than each of these weights if the sound always occurred with the application of the latter while the attention was allowed naturally to fall on the pressures which were to be compared; and second, to find out how many times *S* would be judged heavier than each of these weights under the same conditions as before except that the attention were kept on the sound during its occurrence, but after its cessation transferred to the right hand, so that the sensation from the *S* weight was always received with normal or undistracted attention. In Table VI., which shows what weights were used, the capital letters A, B, C, D, E, serve to designate the couplets of weights compared with the attention resting naturally on the weight

TABLE VI.

| | Subject 6. | | Subject 7. | | Subject 8. | | Subject 9. | |
|---|---|---|---|---|---|---|---|---|
| | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). |
| A | 18/*n* | 20 | 12/*n* | 15 | 10/*n* | 12 | 10/*n* | 12 |
| B | 20/*n* | 20 | 15/*n* | 15 | 12/*n* | 12 | 13/*n* | 12 |
| C | 23/*n* | 20 | 18/*n* | 15 | 13/*n* | 12 | 16/*n* | 12 |
| D | 25/*n* | 20 | 21/*n* | 15 | 15/*n* | 12 | 19/*n* | 12 |
| E | 28/*n* | 20 | 24/*n* | 15 | 18/*n* | 12 | 22/*n* | 12 |
| a | 18/*n* | 20 | 12/*n* | 15 | 10/*n* | 12 | 10/*n* | 12 |
| b | 20/*n* | 20 | 15/*n* | 15 | 12/*n* | 12 | 13/*n* | 12 |
| c | 23/*n* | 20 | 18/*n* | 15 | 13/*n* | 12 | 16/*n* | 12 |
| d | 25/*n* | 20 | 21/*n* | 15 | 15/*n* | 12 | 19/*n* | 12 |
| e | 28/*n* | 20 | 24/*n* | 15 | 18/*n* | 12 | 22/*n* | 12 |

sensations, while the small letters, a, b, c, d, e, designate the couplets in which the attention was voluntarily concentrated on the sound. The sign /*n* stands for the sound.

Of course practice was necessary before the subjects learned to concentrate on the sound (*i. e.*, in the cases of the small letters). They were told that in case such concentration made them unable to judge with confidence, they should mention the fact and withhold their reports if necessary. Their experience was, however, that when the concentration was directed to the sound and the concomitant pressure therefore less strongly attended, there nevertheless remained a good memory image of that pressure which enabled them to judge it in a very satisfactory way.

Subjects 6 and 7 seemed to acquire the needed control of the attention with great readiness. Subject 8 usually reported the control good, but the operator frequently had doubts about the matter.[1] Subject 9 admitted that he was easily distractible by noises and the like occurring while he was studying, and he sometimes stated that his attention to the sound was not good. It was necessary to give emphasized instructions not merely to attend to the sound during the first pressure application but in all cases to return to the second pressure with normal attention, else there was danger that the second weight would be insufficiently attended. In order to stimulate the subjects to proper effort the operator reminded them very frequently (sometimes each time) what was to be done—if only with a word.

In taking the protocol ten capital letters were first given and then ten small letters or *vice versa*, care being taken to equalize conditions. That is to say, ten judgments were passed in succession with the attention voluntarily directed to the sound in each case, and next ten with the attention not on the sound but naturally directed toward the pressures on the right hand which were to be discriminated. Judgment was always passed in terms of the second pressure, and the numbers in Table VII. represent the number of times the second weight was judged heavier.

§12. *Results.*—It will be noticed that the results are very pronounced indeed. The italic numbers show a great increase over the roman. That is to say, the number of times a given weight is judged heavier than another is much increased if the attention is not directed toward the latter. *Decrease of attention to a pressure seems therefore to involve decrease of intensity of sensation.*[2]

[1]At such times on several occasions he stopped taking the records in order to give the subject special practice, a thing also done with subject 9.

[2]Subject 3 also was tried for this series. This subject, however, found the rattling sound of the buzzer very disagreeable and apparently was not able to perform the desired concentration. Hence only 3–A was performed with this subject. It is interesting to state that here A+B+C+D+E=30 while a+b+c+d+e=23½, showing a decrease where with proper attention there presumably would have been an increase. The instruction to concentrate on the sound, instead of being carried out, led to the opposite achievement.

Therefore we are warranted in inferring that in Series 1 and 2 the diminution of the one sensation by another was identical with a distraction of the attention. Or rather, it is more illuminating to put it the other way: *in the case of simultaneous sensations, distraction of the attention is equivalent to diminution or inhibition of one by the other.* Also, some of the subjects reported that attention away from the pressure seemed to give its sensation a changed quality. We may perhaps explain this by remembering the principle elaborated by Heymans that faint sensations may be utterly suppressed

TABLE VII.

| | | Subject 6. | | Subject 7. | | Subject 8. | | Subject 9. | |
|---|---|---|---|---|---|---|---|---|---|
| Series 3–A. | | | | | | | | | |
| A | *a* | 9 | *9* | 8 | *8½* | 6 | *7* | 4 | *7* |
| B | *b* | 5 | *7* | 5½ | *6* | 4½ | *4½* | 6 | *3* |
| C | *c* | 3 | *10* | 6½ | *10* | 4 | *2* | 4 | *7* |
| D | *d* | 4 | *8* | 6 | *8* | 7 | *4* | 4 | *4* |
| E | *e* | 3 | *5* | 5 | *7½* | 4 | *3½* | 2 | *4* |
| Sum | *Sum* | 24 | *39* | 31 | *40* | 25½ | *21* | 20 | *25* |
| Series 3–B. | | | | | | | | | |
| A | *a* | 7 | *10* | 8½ | *7* | 8 | *7* | 6 | *4* |
| B | *b* | 9 | *8* | 3 | *7* | 5 | *8* | 3 | *5* |
| C | *c* | 8 | *9* | 5½ | *9* | 4 | *9* | 2 | *3* |
| D | *d* | 5 | *8* | 5 | *8* | 3 | *6* | 1 | *1* |
| E | *e* | 5 | *2* | 2 | *7* | 2 | *2* | 3 | *3* |
| Sum | *Sum* | 34 | *37* | 24 | *38* | 22 | *32* | 15 | *16* |
| Series 3–C. | | | | | | | | | |
| A | *a* | 7 | *8* | 9 | *9* | 9 | *8* | 7 | *7* |
| B | *b* | 5 | *9* | 6½ | *7½* | 4 | *6* | 5 | *7* |
| C | *c* | 8 | *8* | 5½ | *8½* | 5½ | *9* | 2 | *3* |
| D | *d* | 7 | *7* | 4 | *5½* | 5 | *6* | 4 | *3* |
| E | *e* | 3 | *5* | 1 | *5* | 3½ | *4½* | 0 | *2* |
| Sum | *Sum* | 30 | *37* | 26 | *35½* | 27 | *33½* | 18 | *22* |
| Series 3–D. | | | | | | | | | |
| A | *a* | 7 | *7* | 7 | *6½* | 9½ | *6* | 7 | *6* |
| B | *b* | 9 | *10* | 5½ | *9* | 4 | *5* | 3 | *5* |
| C | *c* | 5 | *7* | 4 | *7* | 6 | *6* | 5 | *7* |
| D | *d* | 5 | *6* | 5½ | *8½* | 5½ | *6* | 3 | *2* |
| E | *e* | 2 | *7* | 4½ | *7* | 3 | *6* | 2 | *2* |
| Sum | *Sum* | 28 | *37* | 26½ | *38* | 28 | *29* | 20 | *22* |
| Total sum | *Total sum* | 116 | *150* | 107½ | *151½* | 102½ | *115½* | 73 | *85* |

Each figure in roman type opposite A or B or C or D or E represents the number of times out of ten that a certain weight *S* was judged heavier than some other one under conditions described in the context. In the space to the right of each such figure in roman is one in italic which represents the number of times out of ten that *S* was judged heavier than that same other weight under the same conditions as before except that the latter weight received less attention.

by other strong ones. Now our pressure sensation was not a simple sensation, but rather a group of sensations—some of different qualities and strengths—some of light surface contact, others of deeper pressure. Therefore when one subject reports that withdrawing the attention from the pressure makes the sensation become 'dull and heavy,' we may reasonably suppose that this is due to the fact that the fainter sensations become suppressed, leaving the stronger ones as the residue and thus changing the character of the whole. It is interesting to think that increasing the attention of such a complex sensation is like increasing the volume of a clang, for thereby one changes not alone its quantity but its quality by making the 'overtone' sensations, so to call them, become more and more perceptible.

## Series 5.

### *The Inhibitory Effect of Inattention on Sound Sensations.*

§13. *Apparatus and Procedure.*—This is in principle the same as 3, but instead of judgments about pressure sensations with sounds as distractives, the judgments were made concerning sounds with pressures for distractives. The same instruments for pressures and sounds were used as those of Series 2, and the sounds here were also one half second in duration. Of course the operator made every effort to observe the caution that the pressure be applied simultaneously with the beginning of the sound. Table VIII. shows the relative strengths of sounds used, and the sign /300 indicates that a pressure of 300 gr. was applied with the first sound.

Table VIII.

| | Subject 6. | | Subject 7. | | Subject 8. | |
|---|---|---|---|---|---|---|
| | First Sound Given. | Second Sound Given. | First Sound Given. | Second Sound Given. | First Sound Given. | Second Sound Given. |
| A | 5. /300 | 8. | 5./300 | 7. | 6./300 | 8.5 |
| B | 6. /300 | 8. | 6./300 | 7. | 7./300 | 8.5 |
| C | 6.7/300 | 8. | 7./300 | 7. | 8./300 | 8.5 |
| D | 8. /300 | 8. | 8./300 | 7. | 9./300 | 8.5 |
| a | 5. /300 | 8. | 5./300 | 7. | 6./300 | 8.5 |
| b | 6. /300 | 8. | 6./300 | 7. | 7./300 | 8.5 |
| c | 6.7/300 | 8. | 7./300 | 7. | 8./300 | 8.5 |
| d | 8. /300 | 8. | 8./300 | 7. | 9./300 | 8.5 |

The numbers stand for strengths of sounds: the smaller the number the louder the sound. The sign (/300) stands for the application of the 300 gr. pressure. In the couplets of sounds designated by the capital letters the attention rests naturally on the sounds to be discriminated. In those designated by the small letters the first sound receives less attention than in the case of the capital letters for it is voluntarily placed on the pressure sensation; but the second sound receives normal attention.

§14. *Results.*—The numbers in Table IX. represent the number of times out of ten that the second sound was judged louder. The results are similar to and in harmony with those of Series 4. The italic figures are in 19 out of 24 cases

TABLE IX.

| | | Subject 6. | | Subject 7. | | Subject 8. | |
|---|---|---|---|---|---|---|---|
| Series 5–A. | | | | | | | |
| A | *a* | 0 | *4* | 1 | *4* | 1½ | *5* |
| B | *b* | 1 | *4½* | 2½ | *8* | 5 | *6* |
| C | *c* | 5 | *6½* | 4½ | *9* | 8 | *9* |
| D | *d* | 7½ | *6½* | 10 | *9* | 8 | *9½* |
| Sum | *Sum* | 13½ | *21½* | 18 | *30* | 22½ | *29½* |
| Series 5–B. | | | | | | | |
| A | *a* | 2 | *2½* | 2 | *3½* | 1½ | *3½* |
| B | *b* | 2½ | *6½* | 1 | *8* | 7 | *7* |
| C | *c* | 7½ | *7* | 7½ | *10* | 8 | *7* |
| D | *d* | 6½ | *9* | 9 | *10* | 9 | *9½* |
| Sum | *Sum* | 18½ | *25* | 19½ | *31½* | 25½ | *27* |
| Total sum | *Total sum* | 32 | *46½* | 37½ | *61½* | 48 | *56½* |

Each figure in roman type opposite A or B or C or D represents the number of times out of ten that a certain sound *S* was judged louder than some other one under conditions described in the context. In the space to the right of each such figure in roman is one in italic which represents the number of times out of ten that *S* was judged louder than that same other sound under the same conditions as before except that the latter sound received less attention.

larger than the roman, which means that a sound is judged less loud when simultaneously the attention is voluntarily directed to something else. If these results be accepted then without doubt it is true that *decrease of attention involves decrease of intensity of sound.*[1]

If so, then we have in this principle the means to explain the well-known phenomenon called the 'temporal error,' namely, that in comparing two successive sensations the

[1]Heymans, also, states that attention to an inhibiting psychic process increases its inhibitory power. *Ibid.*, 53, 1909, 402.

second usually is overestimated. It is obviously due to this that after the subject receives the first stimulus he awaits the second with stronger expectant attention.[1] Similarly, we can readily understand from the principles developed in the above four series why it is true that small differences between weights are much harder to discriminate when simultaneously than when successively given. The sensations from simultaneously given weights inhibit each other and thus forestall anything like the precise discrimination possible with successive sensations.

## Series 6.

### *The Power of Voluntary Attention to Overcome Inhibition.*

§15. *Procedure.*—Since in Series 1 and 2 it had been shown that a sound or pressure sensation may inhibit a pressure or sound sensation it suggests itself to inquire whether by an extraordinary effort to concentrate on the sensation judged the inhibiting influences of the distractor might not be neutralized. One set of observations (80) was taken with subject 7 and two sets with subject 9, sounds being judged. More would have been taken but for derangements in the apparatus and batteries. The following table shows what was done in the case of subject 7.

| | Sound First Given. | Second Sound. |
|---|---|---|
| A | 5 | 6.7 |
| B | 6 | 6.7 |
| C | 7 | 6.7 |
| D | 8 | 6.7 |
| a | 5/300 | 6.7 |
| b | 6/300 | 6.7 |
| c | 7/300 | 6.7 |
| d | 8/300 | 6.7 |

The sign /300 indicates when a 300-gr. weight was given with the first sound. The subject was directed to concentrate very hard on the sound that was accompanied by the pressure.

§16. *Results.*—The results for 80 observations with subject 7 were: A+B+C+D=17 and a+b+c+d=14. These

[1]Sometimes, however, subjective conditions are such as to bring heavier attention and corresponding overestimation to the first stimulus, but this is seldom found to occur for a large number of times in succession.

numbers represent the number of times the second sound was judged the louder. For subject 9 the results for 2 sets of 80 observations each were respectively A+B+C+D=26 while a+b+c+d=27 and A+B+C+D=24 while a+b+c+d=18.5.

These figures mean that the presence of the pressure simultaneous with the first sound did not succeed in causing the second sound to be judged louder more times than when the pressure was absent. In other words the extra concentration on the sound sensation prevented it being diminished by the pressure sensation.

This same compensation was demonstrated by some few observations on pressure sensations with sounds as distractives. 100 judgments by subject 8 and A+B+C+D+E=16.5 while a+b+c+d+e=14.5. A single test made with subject 7 also brings out the augmentor effect of voluntary attention. For this subject we take Series 3-C together with 3-D in Table VII. the figures A+B+C+D+E=52.5. This represents the number of times a pressure sensation was judged heavier than others when the sound of the buzzer occurred with the latter. We found that this number would be increased if the attention was voluntarily directed to the sound. Now a hundred tests were made with the direction to concentrate hard on the first pressure, with which the sound came, and the results were a+b+c+d+e=35.5. In short, voluntary concentration on the first pressure decreases the number of times that the second pressure is judged heavier.

Hence the results indicate that *the influence of an inhibiting agency may be neutralized by the voluntary attention.* It seems perfectly permissible to infer that the same thing could under favorable conditions be done by the involuntary attention, and that in general it it true that *just as decrease of attention involves decrease of intensity of a sensation, so conversely does voluntary or involuntary concentration involve increase of intensity.*

## Series 6.

### *Inhibition of Pressure Sensations by Other Stronger Pressure Sensations.*

§17. *Apparatus and Procedure.*—The writer had commenced his investigations on the inhibition of sensations with a series in which was tested the effect of lowering a 200-gr. weight over a pulley onto the left hand upon the strength of other pressure sensations on the right. The results were not the same for all the subjects, seeming to show an inhibition of the sensations of the right hand for some subjects and an augmentation for others. At that time, however, he did not understand the necessity of keeping the time periods of the inhibiting sensation and of the inhibited sensation in a certain constant relation. Hence it seemed desirable to repeat the test under more exact conditions.

The pressure sensations judged were produced on the right hand with the same apparatus as in Series 1 and the method was the same, except that the pressures lasted only two seconds instead of four. A special arangement was used for pressures on the left hand as shown in Fig. 4. The sub-

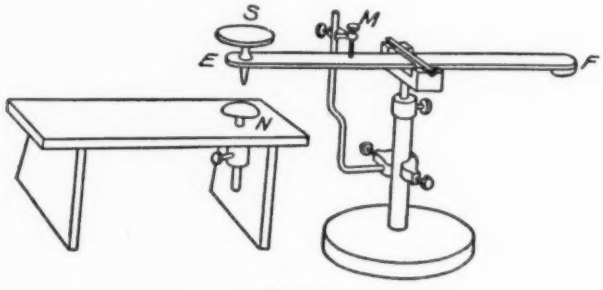



Fig. 4.

ject's left hand was placed palm downwards on the rest *N* and a cork disc again of about 15 mm. in diameter placed upon it. Now the screw *M* was turned down until the pin under *S* was in light contact with the cork. The pan *S* was loaded with a 300- or a 500-gr. weight. The fingers of the operator continually pressed down the lever *E–F* at the point *F*, so that he had but to remove his fingers in order that the

300- or 500-gr. pressure might be applied to the subject's hand.

Great effort was made to apply the pressures to both hands as nearly simultaneously as possible, and though the subjects asserted that they were often apperceived as not simultaneous, we may nevertheless reasonably assume that the success was in the long run satisfactory. Table X. shows the weights used for each subject.

TABLE X.

| | Subject 3. | | Subject 8. | | Subject 1. | | Subject 4. | |
|---|---|---|---|---|---|---|---|---|
| | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). | First Wt. Given (gm.). | Second Wt. Given (gm.). |
| A | 22 | 20 | 18 | 15 | 18 | 15 | 21 | 17 |
| B | 20 | 22 | 15 | 18 | 15 | 18 | 17 | 21 |
| C | 22 | 20/300 | 18 | 15/500 | 18 | 15/500 | 21 | 17/500 |
| D | 20/300 | 22 | 15/500 | 18 | 15/500 | 18 | 17/500 | 21 |
| E | 22 | 24 | 18 | 21 | 18 | 22 | 21 | 26 |
| F | 24 | 22 | 21 | 18 | 22 | 18 | 26 | 21 |
| G | 22 | 24/300 | 18 | 21/500 | 18 | 22/500 | 21 | 26/500 |
| H | 24/300 | 22 | 21/500 | 18 | 22/500 | 18 | 26/500 | 21 |

The signs (/300) and (/500) indicate the application of pressure of 300 or 500 gr respectively. Otherwise the explanation of this table is the same as that of Table I

§18. *Results.*—The results for subjects 7, 8 and 1 show that the 500-gr. pressure did have a predominantly inhibitory effect on the sensations of the right hand. With subjects 3 and 8 this effect was clear from the first. With subject 1 some observations were previously taken where left-hand pressure 200 gr. and at another time left-hand pressure 400 gr. The results were inconclusive. This seemed possibly explanable because subject 1 made such restless efforts to judge the weights with the greatest accuracy and hence perhaps by frequent concentration on the right hand neutralized the inhibiting influence of the pressure on the left. Presuming that this might be the explanation of the case, the operator concluded that it would be necessary to have the weight on the right hand sufficiently heavy to manifest its influence in spite of the restless shiftings of the attention, and therefore he selected the 500-gr. weight. The results then showed, as previously remarked, that this weight had a marked inhibitory influence on the right-hand sensations.

As to the explanation of the results of subject 4 the writer is somewhat in doubt. That subject expressed a great lack of confidence in his judgment in this series, and the operator had similar misgivings. It is possible that the

TABLE XI.

| | | Subject 3. | | Subject 8. | | Subject 1. | | Subject 4. | |
|---|---|---|---|---|---|---|---|---|---|
| Series 6–A. | | | | | | | | | |
| A | *C* | 7 | *10* | 5 | *7½* | 8 | *9* | 3½ | *0* |
| B | *D* | 8 | *8* | 9 | *10* | 9 | *8½* | 7 | *3½* |
| A+B | *C+D* | 15 | *18* | 14 | *17½* | 17 | *17½* | 10½ | *3½* |
| E | *G* | 5 | *10* | 1 | *6* | 3 | *6* | 0 | *0* |
| F | *H* | 5 | *6* | 6½ | *7* | ½ | *4* | 2½ | *1* |
| E+F | *G+H* | 10 | *16* | 7½ | *13* | 3½ | *10* | 2½ | *1* |
| A+B+E+F | *C+D+G+H* | 25 | *34* | 21½ | *30½* | 20½ | *27½* | 13 | *4½* |
| Series 6–B. | | | | | | | | | |
| A | *C* | 5 | *7* | 3½ | *9* | 6½ | *6½* | 6 | *4* |
| B | *D* | 8 | *8* | 9½ | *7* | 7 | *5* | 8 | *4½* |
| A+B | *C+D* | 13 | *15* | 13 | *16* | 13½ | *11½* | 14 | *8½* |
| E | *G* | 2 | *8* | 1½ | *10* | 2 | *6½* | 1 | *0* |
| F | *H* | 3 | *8* | 3½ | *7* | 3 | *3* | 5½ | *7½* |
| E+F | *G+H* | 5 | *16* | 5 | *17* | 5 | *9½* | 6½ | *7½* |
| A+B+E+F | *C+D+G+H* | 18 | *31* | 18 | *33* | 18½ | *21* | 20½ | *16* |
| Total sum | *Total sum* | 43 | *65* | 39½ | *63½* | 39 | *48½* | 33½ | *20½* |

The explanation of this table is the same as that of Table II., except that the distractive used here is a simultaneous pressure of 300 or 500 gr., on the left hand instead of a sound.

strong pressure on the left hand exercised something of the same inhibitory influence in his case that it did with the others, for he reported that when the sensations from the right hand were faint they were sometimes almost crowded out of consciousness by the left-hand pressure. The subject stated that the pressure on the left hand markedly reduced the clearness of the sensations of the right hand. Upon being told of the results he thought them probably due to this unclearness coupled to an associated notion of heaviness whenever the left-hand pressure came. Other explanations, also, would be conceivable, but they only would introduce speculation.

At any rate so much was found to be true in this series: *that the intensity of the pressure sensations of moderate strength*

*may often be diminished by other stronger pressure sensations.*[1]

§19. *Conclusion.*—The results of our experiments have been to show that sound sensations of moderate intensity may be inhibited by simultaneous strong pressure sensations, that pressure sensations of moderate intensity may be inhibited by other simultaneous sensations either of sound or of pressure, that increase of attention to the inhibitor increases its effect, while increase of attention to the other sensation decreases the inhibition. This means that in the case of simultaneous sensations what is commonly known as effective distraction of the attention consists of the inhibitory influence which one sensation exerts upon another; and that degree of (subjective) intensity of sensation is a function of degree of attention. And though Heymans does not take up any special discussion of this last proposition, it can be deduced also from the facts he develops and demonstrates: a sensation to which there is adequate stimulus rises above the threshold in proportion as inhibition of it by other simultaneous sensations is absent. In other words, its intensity increases in proportion as the absence of other sensations permit the stimulus to be more and more attended. In short, the experiments of Heymans and Bentley taken together with our own seem to prove that degree of intensity of sensation is a function not of amount of objective stimulus alone, but of the latter taken together with the influence of other simultaneous sensations.

[1]We need to record in passing that questions about 'clearness' brought no unanimity of answer from the subjects, as previously stated; but it is interesting to note that in Series 4 and 5 where the attention was voluntarily placed on the inhibitor and the intensity of the other sensation much reduced as shown by the reports, yet the two subjects (6 and 7) who had the greatest skill in thus placing their attention agreed that for the most part the sensation not attended to was practically as clear as that upon which attention was directly placed. It was not our attempt, however, to make systematic and exhaustive investigation with reference to clearness and attention; but it seems well to throw out the suggestion here that our conclusion that decrease of attention brings decrease of intensity to sensation does not necessarily conflict with such facts as that distraction by noise during the process of addition brings decrease of clearness to the mathematical material attended to. (Geissler, 'The Measurement of Attention,' *Amer. Jour. of Psy.*, 20, 1909.)

# THE RÔLE OF PITCH IN RHYTHM

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## IMPORTANCE OF QUANTITATIVE METHODS IN THE STUDY OF RHYTHM

Since the appearance, over sixteen years ago, of the pioneer work of Meumann[1] and of Bolton,[2] psychological literature has been enriched by very numerous researches dealing by means of experimental methods with the conditions of rhythm. Among the rhythmical influences most thoroughly studied under this heading have been the relative intensity of the different members of the series of stimuli which gives rise to the consciousness of rhythm, the rate of succession of these members of the stimulus series, their relative and absolute duration, and their pitch. The typical procedure in the investigation of these rhythmical factors has been to present a series of sounds or other stimuli, some of which are longer, more intense, or of different quality than the others, and to ask the observer to describe his impression of the series, especially as regards the way in which the stimuli appear to be grouped. Such a procedure is characteristically a psychophysical one, just as much so as when two weights are presented to an observer for him to lift, and then to describe his impression of them, with special reference to their relative heaviness. In both cases the investigation is one of relationship between certain conditions existing in the environment and the nature of the subject's consciousness under those conditions. Yet accurate psychophysical methods, methods capable of giving quantitative results, have not been used to any great extent in the study of the conditions of rhythm. Hitherto, neither the method of minimal changes

[1]'Zur Lehre vom Zeitsinn,' *Phil. Stud.*, *9*, 1894, 264; 'Untersuchungen zur Psychologie und Aesthetik des Rhythmus,' *ibid.*, *10*, 1894, 249.

[2]'Rhythm,' *Amer. Journ. of Psychol.*, *6*, 1894, 145.

nor that of constant stimuli has been used systematically in the direct investigation of rhythm, that is, in experiments in which the judgment rendered by the subject, instead of being one concerning the heaviness or the brightness of two or more stimuli, is one which concerns the way in which the stimuli appear to be grouped together, a judgment concerning the kind of rhythm of which the subject is conscious.

While much accurate and reliable information about rhythm has been obtained, the lack of quantitative results has hitherto constituted an important defect in our knowledge of the subject. By quantitative results are meant results which show, supposing the same *kind* of rhythm to be perceived under two different sets of conditions, in which case the rhythm is stronger, and how much stronger. One striking consequence of this lack of quantitative results concerning rhythm is, that all data so far obtained on the result of the combination of differences in duration of the members of the stimulus series with differences in either intensity or pitch are of little value; for evidently, the effect of a combination of two or more factors will depend not only on the direction, but, also upon the magnitude of the effect produced by each factor when acting separately. Both the direction and the magnitude of a resultant force depend upon the magnitude as well as the direction of the components. This elementary law, which applies in psychophysics and in psychology as well as in physics, makes it impossible to predict anything very definite concerning the effect of the combination of two or more rhythmical influences, especially influences of different direction, unless there is first known something about the magnitude of the effect of the separate influences. The importance of such quantitative knowledge is well shown, by its absence, in such cases as the following.—Bolton found that for seven subjects out of eight, in case every second sound was longer as well as louder, the longer and louder sound came at the end of the two-membered group.[1] But this result can not be generalized; as I have often found that making every second sound longer and louder *may* bring about the perception of a

[1] *Amer. Journ. of Psychol.*, 6, 1894, 231.

very strong trochaic rhythm, one in which the longer and louder sound *begins* the group.[1] The result that will be obtained depends upon the direction *and magnitude* of the effect produced by the particular intensity and duration used, when each is acting alone. This, in turn, depends upon a large number of factors, including both the absolute and relative intensity and the absolute and relative duration of the sounds composing the rhythmical series.

Another instance of conclusions invalidated by the absence of quantitative results concerning the effect of the individual factors are the conclusions of Squire respecting the combination of differences in pitch with differences in duration.[2] No experiments were made to determine the effect on her subjects of the differences in duration alone. The statement made concerning one subject, that pitch was not only a constant determinant, but also a more effective one, than time,[3] meaning by time the duration of the tones, is unwarranted, because it is not shown that pitch and duration are working against each other, since the effect of the differences in duration which were introduced was unknown. It is quite possible that the differences in duration had no effect whatever upon the rhythm, because the effect of the increase in duration, as such, was possibly cancelled by the increase in apparent intensity which accompanies an increase in duration.[4] This increase in apparent intensity, due to increase in duration,[5] was not eliminated by Squire's method of equating intensities, for this consisted in a comparison by the subject of the

[1] *Archives of Psychol.*, *14*, 1909, Table VI., 36.

[2] 'Genetic Study of Rhythm,' *Amer. Journ. of Psychol.*, *12*, 1901, 546–560.

[3] *Ibid.*, 556.

[4] With sounds of very short absolute duration, this increase in apparent intensity as the result of an increase in duration is very great. Consequently, with sounds of short duration, if every second sound is two or three times longer than the others, it will seem louder; and if the intervals between the sounds are equal, the rhythm will be trochaic. With sounds of sufficiently great absolute duration, and of equal objective intensity, if every second sound is two or three times longer than the others, and the intervals between the sounds equal, the rhythm will be iambic. An intermediate duration can be found where the two effects cancel each other.

[5] Kafka, 'Ueber das Ansteigen der Tonerregung,' *Psychol. Stud.*, *2*, 1907, 256; Sander, 'Das Ansteigen der Schallerregung bei Toenen verschiedener Hoehe,' *ibid.*, *6*, 1910, 142.

intensities when the tones of the different pitches were given for the same length of time, namely, three seconds. It is true that if pitch and duration, when they occur separately, were to work in opposite directions, it would not require quantitative results to determine merely whether a certain difference in pitch could overcome a certain difference in duration. But even supposing that pitch and duration, when occurring separately, had opposite rhythmical effects, quantitative work would be necessary to determine whether they continued to work against each other when occurring in combination. It could not be assumed, without experimentation, that the rhythmical resultant of two such factors is determined by algebraic addition, as in the case of two physical forces working in opposite directions.

Another consequence of the lack of quantitative methods of studying rhythm has been the appearance of a great many contradictions in results obtained from different individuals by the same experimenter. These contradictions are to a great extent only apparent, and arise from the fact that as long as quantity is not taken into consideration, any two magnitudes of opposite signs are apt to be considered as differing widely from each other. A two-membered rhythmical group is either iambic or trochaic. These two judgments seem at first sight to mean radically different things: and yet two persons, one of whom judges iambic and the other trochaic, with respect to the same series of sounds, may be very much nearer to each other in their manner of perceiving the rhythm than two persons both of whom judge trochaic,—just as the difference between minus and plus one is less than that between plus one and plus one hundred. I have found, using the quantitative method outlined below, that two persons who disagree with regard to the quality of the rhythm may sometimes be made to agree by an extremely slight change in the rhythmical series presented to one of them, whereas, a considerable change must sometimes be made for one of the subjects, before two of them, both of whom originally judged trochaic, can be made to get an *equally strong* trochaic. Now, judgments concerning one's consciousness of

rhythm, like any other psychological processes, are variable; and this variability detracts from the probability that a judgment made only once will be repeated under the same experimental conditions. In view, then, of the possibility of the existence of a very small difference between the significance of the judgments trochaic and iambic, and the variability of all judgments concerning rhythm, it is a highly unreliable conclusion, where we have only one judgment made by each of a number of individuals, some of whom judge trochaic and some iambic, that there is any significant difference between the individuals in question. To understand the import of this, it should be remembered that the procedure in many experiments on rhythm consists in placing before an individual a series of sounds and letting that series continue until the subject makes up his mind concerning its rhythmical nature. The series being continuous, and the subject knowing that it has not been objectively changed, even though there be some fluctuation in the subject's mind before he arrives at a decision, there is obtained from him only one judgment.

Such a difference as that just referred to in the judgments of a number of individuals is frequently met with. Thus, in the experiments Bolton made with the view of investigating the rhythmical effect of differences in duration of the sounds and of differences in duration combined with differences in intensity, in no case did all subjects agree, though the number of subjects ranged only from four to ten, and in one case 'the preferred rhythm' was preferred by only four subjects out of seven, and in another by two out of four.[1] That this contradiction in the judgments of individuals is due to the failure to employ quantitative methods, is indicated by the fact that, by using the latter I was able to generalize the rhythmical effect of duration in the case of thirteen subjects in such a way that there were no individual exceptions.[2] Again, Squire found that when she presented an evenly spaced series of sounds of equal intensity and duration, in which every second or every third was of a different pitch from the others,

[1] *Amer. Journ. of Psychol.*, *6*, 1894, 231.

[2] 'A Quantitative Study of Rhythm,' *Archives of Psychology*, *14*, 1909, 64–66.

that the subjects, though only seven in number, fell into three classes,—those who accented the high tones, those who accented the low, and those to whom highness and lowness were a matter of indifference.[1] Squire concludes from her experiments that for certain individuals pitch has great effectiveness as a determinant of rhythm.[2] The results presented below, however, obtained from thirteen subjects, show that had the same methods been used as those employed by Squire, about the same results would have been obtained; but show further, in a conclusive manner, that pitch cannot in the case of a single subject be a reliable or constant determinant of rhythm. The reason for the greater uniformity and the absence of individual exceptions in the results reached in the present investigation, is to be found in the use of quantitative methods. As will be explained later, the amount of rhythm occurring when differences in pitch only are present is always exceedingly small, so small that many judgments are necessary to establish its reliability. But when these many independent judgments are obtained, it is found that the direction and quantity of the rhythm vary about zero so closely and so much as though by chance that the final conclusion can be only that pitch differences do not determine the rhythm at all.

## General Survey of Experimental Results on Pitch as a Condition of Rhythm.

It is unnecessary to review all the widely diverging opinions concerning the effect of quality in rhythm, as most of them do not even make any pretense of being based on experimental data. In 1894, Bolton expressed the prevailing opinion, which was practically without experimental foundation, as follows: "Qualities of sounds are quite as important as unifying elements as their time and intensity relations, and were quite as early regarded." [3] Since then, the only study throwing much light upon pitch as a rhythmical factor is that by Squire. The results that have been obtained on

[1] *Amer. Journ. of Psychol.*, *12*, 1901, 556.
[2] *Ibid.*, 549–558.
[3] *Amer. Journ. of Psychol.*, *6*, 1894, 162.

rhythmical speech are hardly pertinent here. It is indeed true that a relationship has been found to exist between accent and pitch, the accented sound having generally a higher pitch than the unaccented.[1] But the same is true of prose, and may be regarded merely as a matter of ease of enunciation.[2] It is also true that occasional observations have been made, incidentally and in a loose way, which suggest some effectiveness on the part of pitch as a determinant of rhythm. Such observations hardly require criticism after what has been said of the importance of metric methods in the study of rhythm. MacDougall observed that differences in the quality of sounds produced a change in the rhythmical impression, that is, a change in the apparent grouping; and consequently he regards quality as a determinant of rhythm.[3] These observations were made incidentally in the course of his investigation of the effect of intensity of sounds in rhythm, the differences in quality occurring merely as the result of imperfections in his apparatus. In describing this apparatus, which consisted in a device for dropping metal weights in succession from adjustable heights, he writes: "The hammers and anvils, though fashioned from the same stuff and turned to identical shapes and weights, could not be made to ring qualitatively alike; and these differences, though slight, were sufficiently great to become the basis of discrimination between successive sounds and of the recognition upon their recurrence of particular hammer-strokes, thereby constituting new points of unification for the series of sounds. When the objective differences of intensity were marked, these minor qualitative variations were unregarded; but when the stresses introduced were weak, . . . they became sufficiently great to confuse or transform the apparent grouping of the rhythmical series; for a qualitative difference between two sounds, though imperceptible when comparison is made after a single occurrence of each, may readily become the subconscious basis for a unification of the pair into a

[1]Miyake, 'Researches on Rhythmic Action,' *Studies from the Yale Psychological Laboratory*, 1902, 1–48.

[2]Squire, *op. cit.*, 557.

[3]Psychol. Review, Monog. Sup., *4*, 1903, *Harvard Psychol. Stud.*, I., 317.

rhythmical group when several repetitions of them take place."[1] MacDougall says nothing as to the kind of effect a higher or lower pitch has, merely that difference in quality may change the expected grouping. But it is certain, if the different weights, when dropping from the same height, produced different qualities of sounds, that they also produced sounds of different loudness; so that when the difference in height was small, the hammer dropping the lesser height may in some instances have produced the louder sound.

Squire, in a carefully conducted investigation, came to the conclusion that for the majority of her subjects pitch was a determinant of rhythm. "While there was no general constancy in the accentuation of the high or low tones, *i. e.*, no constancy for *all* individuals, there was a marked constancy in the interpretation of the *same* individual throughout the different series given."[2] As a matter of fact, Squire's tables, when studied in detail, show that for the majority of her seven subjects there was no constancy in the interpretation of the different series.[3] Her conclusion, that with some subjects pitch is a more effective determinant of rhythm than time, has already been referred to. Squire believes, however, that quality as such is not an independent determinant of rhythm, but owes its effectiveness to associations. "It can be considered as a substitute for or as an intensification of intensity,—whether because of strain sensations common to both, or because of associations which, reduced to their ultimate grounds, are of an intensive character."[4] The introspections which Squire has recorded, rather than bearing

[1] *Ibid.*

[2] *Op. cit.*, 549.

[3] *Op. cit.*, 550–555. Subjects W.M., E.P., B.B. and I.M. did not maintain throughout the different combinations groupings beginning with either the higher or the lower pitched sound, but sometimes obtained groups beginning with the higher pitched and sometimes groups beginning with the lower pitched.

[4] *Op. cit.*, 558. Compare with the opposite view of Ettlinger: "Beim Wechsel verschiedener Intensitaeten draengt sich die rhythmische Bewegungsvorstellung viel zwingender auf, als beim Wechsel der Tonhoehe. . . . Es waere doch irrtuemlich, die stellvertretende Wirkung der Rhythmusurachen so aufzufassen, als wuerden die anderen erst durch Vortaeuschung der Intensitaetsstufen hindurch Ursachen des Rhythmus." *Zeitschrift f. Psychol. u. Physiol. der Sinnesorgane, 22*, 1900, 180–181.

out the interpretation she gives to her results, as being due to associations with intensity, make this interpretation impossible. For example: F.W., "who was fully determined to accent the higher," gives the introspection: "The two tones take the accent equally well. . . . I can change the accent at will": "Higher is first, it is longer, a dragging tone. The lower is more concise, it is accented." (This subject is one of those who is said to have mistaken an *a* of 453 as higher than a *c* of 512.) Now, how can it be argued that the higher pitched sound always came first in the group because of associations with intensity when the introspections show that the lower took the accent as easily as the higher? And after all, was not all influence due to apparent intensity or to associations between pitch and intensity eliminated by the method of equating the intensities of the sounds at the beginning of the experiment? This consisted in regulating their intensity so that the sounds of different pitch seemed equally loud to the subject. No associations with intensity are suggested which would be effective in rhythm, which would not be equally effective while the sounds were being equated as regards apparent intensity and so eliminated; and it seems likely that if such associations had existed, they would have been mentioned in the introspections, as the latter were, apparently, the object of considerable care.[1]

Squire's method, which consisted in always using series in which the sounds were separated by equal intervals, is not a reliable one, as has been already explained. With any one

[1]Squire's interpretation of her results shows considerable similarity to Meumann's position. Meumann, while believing that differences in quality may be regarded as independent causes of rhythm, (*Phil. Stud.*, *9*, 1894, 305) the same as any periodically recurring differences, says, that it seems to him that the importance of certain tones in rhythmical series depends chiefly on the stronger beat and the retarded tempo (*ibid.*, 307). He explains the aid to 'subjective' rhythm which he supposes to be produced by differences in pitch as due solely to apparent differences in the intensity and duration resulting from the differences in pitch. Melody comes in as a rhythmical factor, first, as a result of the greater intensity and duration of certain tones, and second, from 'Innere Zusammenhang.' The latter, in musical rhythm, apparently corresponds with the factor which Meumann distinguishes in poetical rhythm as the "Logischen Zusammenhaengen zu Saetzen." Warner Brown says that it is preposterous to suggest the latter as a rhythmical factor. ('Time in English Verse Rhythm,' *Archives of Psychology*, *10*, 1908, 72.)

series, only one judgment was obtained. It is true that several different combinations were used, such as series in which every second was higher, every third higher, or every third lower. That some of the subjects kept to the same sort of grouping throughout all the different combinations is not however out of keeping with the interpretation, necessitated apparently by the results presented below, that pitch is not a reliable determinant of rhythm. After the subjects had once decided that the higher ended the group, in one of the series, some of them would naturally be inclined to give the same judgment in other series, unless a series was given in which it was plain to them that the higher sound ended the group, so that they would be awakened to the necessity of making new decisions independent of their past ones. The results below show that the amount of rhythm obtained, when no differences but those of pitch exist in the sounds, is always so small, that, in view of the variation existing between successive judgments, when made independently, a large number of independent judgments would be necessary to establish a significant difference between those who judge higher first and those who judge higher second, concerning the same rhythmical series.

Squire says that, one great difficulty in interpreting her results was, the determination whether subjectively high and low corresponded to, objectively high and low. She states that when confusion of judgment was found in the three-group, she presumes that it was present also in the two-group.[1] But it is nowhere explained how this confusion was straightened out. That it never was, is indicated by the almost incredible fact that five of her seven subjects are said to have either been unable to distinguish the difference between *a* of 453 vibrations and *c* of 512 vibrations, or, to have mistaken the higher for the lower,[2] and one subject is said to have found no difference in pitch between a *c* of 256 vibrations and an *a* of 453 vibrations. As these were the pitches used in Squire's investigation, how could she possibly hope

[1] *Op. cit.*, 557.

[2] *Op. cit.*, 550–555. Subjects B.B., C.S., E.P., I.M., and F.W.

to determine the effect of pitch in rhythm with such subjects?

This brief survey of the actual facts at hand concerning the rôle of pitch in rhythm is sufficient to show that my own conclusion, that pitch is not a determinant of rhythm, is not at variance with any well established position. The conclusions of Squire are unwarranted as well as undecided, and this, together with the negative effect of pitch on the estimation of time intervals,[1] enables me to say that my conclusion is, if anything, more in keeping with previous knowledge than at variance therewith.

## Apparatus and Procedure.

The apparatus that I have used for the production of rhythmical series of sounds, while complicated, has several advantages over that used by previous investigators. It permits of independent variation of the duration of intervals and sounds, of the rate at which the series is run off, of the number of sounds contained in a measure, of the intensity and pitch of any sound, of accurate measurement and control of all these variables, and of the ready substitution of electrical shocks, as stimuli, in place of sounds.[2] The sounds used in the present research were produced by passing an electric current, made intermittent by means of an electrical tuning-fork, through a telephone receiver.[3] The diaphragm of the telephone receiver vibrates at a rate identical to that of the tuning-fork used. A Meumann's time-sense machine[4] was used for the purpose of making and interrupting the intermittent current. The time-sense machine permits of

[1]Shaw and Wrinch, *University of Toronto Studies, Psychological Series*, 1899. These authors, in a carefully conducted set of experiments, sought to determine by means of tapping movements "the degree of accuracy that obtains in the reproduction of complex rhythmically arranged groups of intervals of unequal length." The length of the intervals was found to be unaffected by the differences in the pitch of the tones. Sears, *Amer. Journ. of Psychol.*, 1902, 33, states that pitch of tones has no effect on the relative length given to intervals by the performing musician.

[2]For a more detailed account of this apparatus, with diagram and photograph, see 'A Quantitative Study of Rhythm,' *Archives of Psychology, 14*, 1909, 12–29.

[3]This arrangement for producing sounds of different pitch has been recently used by Paul Sander, 'Das Ansteigen der Schallerregung bei Toenen verschiedener Hoehe,' *Psychol. Stud., 6*, 1910, 1–38.

[4]Described in *Phil. Stud., 12*, 1896, 142–152.

variation in length of tones and the intervals between them, variations which to a large extent may be made independently of each other. The durations of intervals and tones were measured by placing in circuit with the telephone receiver a time-marker which wrote on a smoked drum alongside of a tuning-fork of 100 vibrations per second. The time-sense machine was run by an ordinary fan motor and several speed reducers. The rate of rotation obtained in this manner was more constant than that obtained by means of the Helmholtz motor.[1] The intensity of the sounds produced by the telephone receiver was regulated by means of shunts and resistances. To obtain sounds of different pitch, the circuits passing through different contacts on the Meumann's machine were passed through tuning-forks of different pitch. In this way, every second or every third sound could be made higher or lower than the others. The three tuning-forks which were used had pitches of 440, 250, and 50 double vibrations per second. Each tuning-fork was provided with two platinum contacts, one for the circuit which kept the fork vibrating, and one for the sound circuit which passed through Meumann's machine and the telephone. With this arrangement, there was no noticeable click either at the start or end of the sounds. Both of the tuning-fork contacts required very careful adjustment, but it was found possible to produce fairly musical tones, of distinct pitch. Resonators were unnecessary, and were not used. In purity, the sounds were about equal to voice tones as produced by a good phonograph. One of my subjects was a musician, and she was able to identify the pitch of the different sounds very closely, for instance, calling the sound produced by the 250 fork *c'*. She had, further, no difficulty in reproducing by voice either of the two higher tones used. All of the subjects, as well as Professor Pillsbury and Dr. Shepard of the psychological department of the University of Michigan,[2] said that the tones had a distinct

[1]For measurements of the constancy of the rate of rotation with the Helmholtz motor, see *Archives of Psychol.*, *14*, 1909, 16.

[2]This research was carried on, during the summer of 1909, in the psychological laboratory of the University of Michigan, all the ample facilities of which Professor Pillsbury was kind enough to throw open to me. I take this occasion to thank Professor Pillsbury and Dr. Shepard for their invaluable help.

pitch; and those who had had any experience with musical instruments said they thought they could tune an instrument to any of the tones used. One of them tuned a tuning-fork to the sound of 50 double vibrations per second, and missed it by 4 vibrations per second. One subject said of the 440 tone that it sounded like a tuning fork, and another, of the 50, that it resembled in quality the tone of a violincello. Further, though the majority of the subjects were not especially musical, none of them ever confused the pitch of the tones.

The sounds of different pitch were equated for intensity by the subject at the beginning of each sitting. This was necessary, as between sittings the condition of the platinum contacts would change slightly as the result of slight jars to the floor of the room, oxidation, etc. Further, during the middle of each sitting, the rhythmical series was stopped and the tones given in unrhythmical succession to see if they still appeared of equal intensity to the subject. Or, if at any time the subject suspected that the tones were not of the same intensity, the rhythm was stopped and he was given a chance to readjust their intensity. The method used in equating the intensities was about the same as that used by a musician in tuning one tone to another, except that the tuning was for intensity and done by sliding back and forth the slide of a wire resistance-coil. During this act of adjustment by the subject, the tones were not given fast enough nor regularly enough to permit of any rhythmical effect. Thus, throughout the whole investigation, the different tones used all had equal apparent intensity, and the effect of pitch on apparent intensity was eliminated.

The method used in the present investigation rests upon the fact that it is possible to pass from one rhythmical grouping to another by changing the relative duration of the intervals between the sounds. Thus, a rhythm in which the louder, longer, or higher pitched sound begins the group, may be changed to one in which that sound ends the group by increasing sufficiently the interval following that sound, and vice versa. In twenty-five subjects, I have found no exception to this statement. We pass in this way by small

steps from rhythmical groups beginning with a certain sound, to groups ending with that sound, and through a zone where the tendency towards the two forms of grouping is equally strong, or where the tendency to group disappears. The middle point of this zone may be termed the rhythm indifference point. If the rhythm indifference point occurs where all the intervals are objectively equal, then any differences which may exist in the objective duration, intensity, or quality of the sounds are obviously not exerting any effect towards rhythmical grouping. But if, when the intervals between all the sounds are objectively equal, grouping is still perceived, the grouping can not have been occasioned by objective differences in the intervals, and must be due to the presence of other rhythmical factors. The amount of this grouping effect, due to rhythmical factors other than unequal intervals, can be measured by determining the amount by which, *measuring from the point at which all intervals are equal*, the internal intervals, or intervals within the group, must be increased or decreased with respect to the external intervals, or the intervals separating the groups, in order to cause a disappearance of the rhythm, that is, a disappearance of apparent grouping. In short, the amount of rhythm is measured by the amount of change that has to be made in the intervals in order to cause a disappearance of the rhythm. This may be determined either by the method of minimal changes or by that of constant stimuli. In the present investigation, the method of minimal changes was used exclusively.

The subject sat in a room separate from that containing all apparatus but the sound-producing telephone and a sliding resistance, by means of which the subject regulated the intensity of the sounds, in the manner already described. The subject was first given a series of sounds concerning the rhythmical nature of which there could be no doubt, one which he could judge to be plainly iambic, or composed of groups with the higher pitched sound coming last, or with the higher pitched first, etc. This series was allowed to run along uninterrupted until the subject indicated by a bell

signal that he had arrived at a decision. If the subject signalled that he had perceived the rhythm as composed of groups beginning with the louder sound, the interval following the louder sound was slightly lengthened, and that preceding shortened by the same amount. This second rhythm was then given and continued until the subject came to a decision. This procedure was continued until finally the subject began to judge the group to end with the louder sound, in other words, until the rhythm had shifted over from trochaic to iambic. The size of the steps, that is, the amount by which one of the intervals was shortened and the other lengthened was either 3, 4, or 5 times 0.0083 sec., the latter value being that of one degree on Meumann's machine, at the rate of revolution used; but the same sized steps were used throughout any one series. The usual precautions in applying the method of minimal changes were taken, as well as certain additional ones, made necessary by the peculiar nature of the material worked with.[1] In determining any one indifference point, a number of such series of minimal changes, going in both directions, was used.

## Results.

The procedure above outlined is applicable to the investigation of variations in intensity, duration, pitch, or any combination of these factors, and of variations in rate. In order to bring out the significance of the data, presented below, on the effect of pitch, it is necessary first to indicate briefly the result obtained by using such a procedure, on the effect of intensity, duration, and rate. As regards rate, it was found that the amount of rhythm produced by objective differences in duration or intensity, measured by the difference in the intervals at the indifference point, remained constant, relative to the duration of one measure, in two-group rhythms, for rates varying approximately from one to four seconds for one measure.[2] Consequently, results

[1] See *Archives of Psychol.*, *14*, 1909, 20–29.

[2] By *measure*, is meant the time from any point in one group to the corresponding point in the following group, *e. g.*, when every second sound is longer, from the beginning of one of the longer sounds to the beginning of the following longer sound.

obtained at a rate of 1.5 seconds for one measure, the rate used in the present investigation, hold, proportionally to the rate, for series run off either slightly faster or several times slower. At rates slower than one measure in four seconds, there was a gradual falling off in the amount of rhythm present, until at a duration of the measure of from seven to ten seconds, it had nearly or quite disappeared. The fastest rate used was one sound every fourth of a second. There was no tendency whatever for rhythms which were trochaic at the slow or medium rates to become iambic with the fast rates, as claimed by Wundt.[1] With rates of one second or less for a two-beat measure, the rhythm was in every case just as strongly trochaic, or else more so, than with rates of from 1.5 seconds or longer for a two-beat measure.[2] Regarding intensity, it was found that in rhythmical series in which every second or every third sound was more intense, that with an increase in the ratio of the intensity of the louder sound to that of the weaker, there is an increase, first rapid, and then slow, in the tendency of the more intense sound to begin the group. As regards duration, with an increase in the duration of the longer sound to that of the shorter, there is an increase in the tendency of the longer sound to end the group or a decrease in its tendency to begin the group. I had one subject with whom there was only such a slight decrease in the tendency of the longer sound to begin the group, as its length was increased, that I was not certain but that he offered an exception to the above generalization. I have since been able to experiment further with him, however, and find that with sounds of greater absolute duration, he conforms very decidedly to the above generalization. These new results, presented in Table I., not only indicate in a typical manner the effect of increasing the duration of every second sound, but also confirm my conclusion concerning the effect of an increase in *absolute* duration, namely, that with a constant ratio between the duration of the longer and shorter sounds, as their absolute duration increases, there is a decrease in the tendency of the

[1] 'Grundzüge der Physiologischen Psychologie,' 5th ed., 1903, Vol. III., 99.

[2] *Archives of Psychol., 14,* 1909, 34–37.

longer sound to begin the group or an increase in its tendency to end the group. This is brought out by the fact that in the previous work with this subject, in which the shorter sound had always a duration of 0.13 second, no matter if I made the longer sound 6.5 times as long as the shorter, so long as the intervals between the sounds were objectively equal, the rhythm never quite went over to iambic, but merely became somewhat less pronouncedly trochaic. In Table I., however, it is shown that when the absolute duration of the shorter sound is increased to 0.45 second, that by the time the ratio of the duration of the longer to the shorter sound has reached three to one, the rhythm is slightly iambic, and becomes markedly so with a ratio of four to one. The change from trochaic to iambic grouping is indicated in the table by the change in sign of *A–B* from plus to minus.

TABLE I.

THE EFFECT OF DURATION ON THE IAMBIC-TROCHAIC INDIFFERENCE POINT.

Subject, Dy. Duration of shorter sound, constant, = 0.45 second. Duration of one measure, constant, = 3.0 seconds.

| Longer. | Before. | After. | *A–B.* | *MV.* | *N.* |
|---|---|---|---|---|---|
| .60 | .916 | 1.034 | +.118 | .04 | 18 |
| .80 | .846 | .904 | +.058 | .03 | 16 |
| 1.20 | .682 | .668 | −.014 | .02 | 18 |
| 1.60 | .515 | .435 | −.080 | .02 | 15 |

*N* = number of series of minimal changes used in the final determination of any one indifference point, the results of the separate series being averaged.

*MV* = mean variation in the indifference points obtained by the different series of minimal changes.

After = duration in seconds of interval after longer sound, and Before = duration in seconds of interval before longer sound, *at the rhythm indifference point.*

*A–B* = interval *after* minus interval *before*. When *A–B* is positive, it shows that with equal intervals the rhythm is trochaic (longer sound beginning the group) and the amount of *A–B* shows how much the intervals had to be changed from objective equality in order to arrive at the indifference point, *i. e.*, in order to destroy the trochaic rhythm. When *A–B* is minus, it shows that the rhythm is iambic with equal intervals, etc.

Longer = duration of longer sound.

After the explanations that have already been given in connection with Table I., the following tables on the effect of differences in pitch will be readily understood. The duration

of one measure is the time that elapses from the beginning of one of the higher pitched sounds to the beginning of the first succeeding sound of that same pitch. In the case of every third sound lower and the other two higher, for example, it is the time from the beginning of any one of the lower to the beginning of the following lower. The duration of the whole measure remains the same throughout the investigation of any given combination of pitches. Under *Higher* and *Lower* are given the pitch of the higher and the lower tones. By *A–B* is meant the interval after the highest pitched minus that after the lower pitched, each interval being given the length it is found to have, on the average, at the rhythm indifference point. By intervals, is meant the intervals of silence between the sounds. All intervals and durations are expressed in seconds. *A–B* expresses the difference from objective equality of intervals that is necessary in order to destroy whatever rhythm is produced by the differences in pitch. The indifference point is determined, as above described, by passing by small steps, back and forth, from rhythms with the highest sound beginning the group to rhythms with the highest sound ending the group, and taking the point where both groupings seem equally natural or where no grouping at all is obtained. The number of such *series* of small steps, or minimal changes, used in determining any one indifference point, is indicated under *N*. Each series of minimal changes, of course, passed through the rhythm indifference point, and so gave a value for *A–B*. If, then, for example, sixteen series were used in any one case, the value of *A–B* given is the average of the sixteen values obtained by the sixteen series; and the *MV* given is the mean variation of these sixteen values of *A–B* about their average.

Some explanation is necessary as regards the sign of *A–B*. This magnitude, it must be remembered, shows how much the intervals need to be changed from objective equality in order to arrive at the point where grouping disappears, and so is a measure of the grouping effect of whatever rhythmical conditions may be present in the stimulus series. Now, the change from objective equality may have to be made in either of two

directions. If every other sound is louder, the interval after the louder sound has to be lengthened to arrive at the rhythm indifference point, so that when that point is reached, the interval after the accented sound is longer than that before it; in other words, *A–B* is positive. It is of course purely arbitrary which interval is subtracted from the other. I have reckoned the difference between the intervals by subtracting the interval before the loudest, longest, or higher pitched sounds from the others. In Tables II., III., and IV., a positive value of *A–B* means a group-beginning tendency on the part of the higher pitched sound; a negative value of *A–B*, a group-ending tendency on the part of the higher pitched sound. Thus, when *A–B* is positive, it means that the higher pitched note is holding the following lower pitched note or notes to it, so that in order to destroy the rhythm the interval immediately following the higher pitched has to be lengthened as compared with the interval preceding it. When

## Table II.

### The Effect of Pitch in Two-Group Rhythms.

Duration of one measure, constant, = 1.5 seconds. Duration of every sound = 0.2 second. Duration of intervals, when equal, = 0.55 second.

| Subject. | Higher. | Lower. | *A–B.* | *MV.* | *N.* |
|---|---|---|---|---|---|
| F Rr | 440 | 250 | −.04 | .04 | 16 |
| Ce | | | −.02 | .03 | 16 |
| Hr | | | −.02 | .03 | 19 |
| Dy | | | .00 | .03 | 44 |
| B Rr | | | .00 | .02 | 19 |
| Cr | | | +.01 | .05 | 45 |
| Mr | | | +.02 | .02 | 18 |
| Ww | | | +.02 | .01 | 37 |
| Bs | | | +.03 | .01 | 38 |
| Be | | | +.04 | .03 | 18 |
| Ww | 440 | 50 | −.02 | .01 | 18 |
| Dy | | | −.02 | .02 | 21 |
| Ce | | | .00 | .03 | 20 |
| B Rr | | | +.02 | .03 | 34 |
| Bs | | | +.03 | .03 | 22 |
| Mn | 250 | 50 | −.02 | .02 | 18 |
| Bs | | | −.01 | .01 | 14 |
| Hn | | | .00 | .02 | 19 |
| Dy | | | .00 | .02 | 20 |
| B Rr | | | +.02 | .04 | 22 |
| Ww | | | +.02 | .01 | 23 |

## TABLE III.

THE EFFECT OF PITCH IN THREE-GROUP RHYTHMS.

Duration of one measure, constant, = 2.0 seconds. Duration of every sound = 0.13 second. Duration of intervals, when equal, = 0.53 second.

*(A) Groups of one lower and two higher pitched sounds.*

| Subject. | Higher. | Lower. | *A–B.* | *MV.* | *N.* |
|---|---|---|---|---|---|
| Ww | 440 | 250 | +.04 | .02 | 17 |
| Bs | | | +.02 | .06 | 13 |
| Ce | | | +.02 | .03 | 13 |
| Dy | | | +.01 | .02 | 27 |
| B Rr | | | −.02 | .02 | 18 |
| Bs | 440 | 50 | +.02 | .04 | 21 |
| Dy | | | .00 | .03 | 20 |
| B Rr | | | −.02 | .02 | 17 |
| Ww | | | −.03 | .02 | 15 |
| Ce | | | −.08 | .02 | 16 |

*(B) Groups of one higher and two lower pitched sounds.*

| | | | | | |
|---|---|---|---|---|---|
| Bs | 440 | 250 | −.01 | .05 | 18 |
| Ww | | | .00 | .01 | 17 |
| B Rr | | | .00 | .03 | 19 |
| Dy | | | +.01 | .02 | 20 |
| Ce | | | +.04 | .04 | 20 |
| Bs | 440 | 50 | −.14 | .03 | 14 |
| Ww | | | −.06 | .01 | 10 |
| Dy | | | −.01 | .02 | 30 |
| B Rr | | | .00 | .03 | 19 |

*A–B* is negative, it means that with equal intervals the grouping is lower note first and higher last, so that the interval preceding the higher must be increased in order to make the grouping disappear. In the case of groups of one low and two high tones, a positive *A–B* means that with equal intervals the two higher notes begin the group; a negative, that they end it. Similarly, in the case of groups of two low tones and one high one, a positive value of *A–B* means that with equal intervals the high tone begins the group; a negative, that the high tone ends it. Rhythmical groups of the form high-low-high or low-high-low were never obtained except at, or very near, the rhythm indifference point, where almost any rhythm that the subject tries for, or none at all, is equally natural.

In interpreting these results, it is necessary to keep in mind the results already referred to as obtained by the same

method in the case of intensity and duration, and also the significance of the value *A–B*. As already explained, the quantity *A–B* serves as a measure of the quantity of rhythm produced by whatever rhythmical factors are present in the series. In rhythmical series containing sounds of different intensities or of different durations, in many cases the quantity *A–B*, that is, the difference between the intervals preceding and following the louder or longer sound at the indifference point, was very large. In some rhythms produced by alternations of more intense with less intense sounds, I have found the interval after the more intense to be more

TABLE IV.

SHOWING FLUCTUATION IN THE VALUE OF *A–B* WITH DIFFERENT COMBINATIONS OF PITCHES.

| Objective Form. | 1 | 2 | 3 | 4 | 5 | 6 | 7 | Av. *A–B*. | Av. *MV*. |
|---|---|---|---|---|---|---|---|---|---|
| Subject: B Rr | .00 | +.02 | +.02 | −.02 | −.02 | .00 | .00 | .000 | .027 |
| Ww | +.02 | −.02 | +.02 | +.04 | −.03 | .00 | −.06 | −.004 | .013 |
| Ce | −.02 | .00 | | +.02 | −.08 | +.04 | | −.008 | .030 |
| Bs | +.03 | +.03 | −.01 | +.02 | +.02 | −.01 | −.14 | −.009 | .033 |
| Dy | .00 | −.02 | .00 | +.01 | .00 | +.01 | −.01 | −.001 | .023 |

The numbers given to the objective forms follow the order of Tables II. and III. Thus objective form no. 1 is an alternation of the two pitches 440 and 250; form no. 2, an alternation of 440 and 50; no. 3, an alternation of 250 and 50; no. 4, every third sound 250, the others 440; no. 5, every third sound 50, the others 440; no. 6, every third sound 440, the others 250; no. 7, every third sound 440, the others 50.

than 50 per cent. longer than that before it, at the rhythm indifference point. The average size, for four subjects, of *A–B* in the case of the greatest intensive differences I used was 40 per cent. of the interval *B*, which means that the interval before the accented sound was 0.50 second and that after it 0.70 second, at the rhythm indifference point. In rhythms produced by an alternation of a longer and a shorter sound, the interval before the longer had, in some cases, to be made over 100 per cent. longer than that following it, in order to arrive at the indifference point. In one case, it was 135 per cent. longer. The results obtained when only pitch differences were present offer a striking contrast. For two-membered groups, the greatest size obtained by *A–B*

was about 8 per cent., in once case +8 per cent. and in another —8 per cent., of the interval before the higher pitched sound. In 18 cases out of 21, it was less than 4 per cent. In both Tables II. and III., that is, in both two-membered and three-membered groups, $A-B$ is zero more often than anything else. This means that more often than anything else no tendency towards either form of grouping existed when the objective intervals between the sounds were equal. In the remaining cases, $A-B$, and consequently the amount of rhythm found to exist when the intervals were equal, was very small. No one could expect the value of $A-B$, in view of its variability, to come out exactly zero in every case, even supposing that were its true value. Yet, 75 per cent. of the values of $A-B$ found in Tables II. and III. are less than the size of the corresponding mean variations.

Moreover, not in the case of a single subject was there uniformity in the sign of $A-B$. This is shown in Table IV., which presents the results obtained on the five subjects who judged of more than one type of series. This table shows that not only was the amount of rhythm always small, but that the direction of the grouping effect varied for the same subject in the different pitch combinations. For the purpose of determining whether or not a higher pitched tone occurring every second or every third sound has a group-ending or a group-beginning tendency, it is legitimate to average the results obtained with the same subject in the different combinations. I have arbitrarily called the higher-beginning tendency the positive direction, and the lower-beginning tendency the negative. That a higher or lower tone has no general tendency in the case of any of these five subjects to begin or end the group is shown by the fact that the average of the pitch effect in the seven different combinations comes out in the case of each subject practically zero,—in every case it is less than 0.01 second and at least several times less than the average mean variation for all the different combinations. The result is the same, if, instead of considering as positive the higher-beginning tendency, we treat the results in any other way. For instance, we may consider the question

as to whether, in three-group rhythms, that tone which is of a different pitch from the other two, irrespective of whether it is higher or lower, has not either a group-beginning or group-ending tendency. But from whatever angle the results are viewed, the fact remains that the differences in pitch are always productive of practically no rhythm, and no general tendency whatever exists as to the direction of the insignificant amounts of it that are produced.

The very slight indications of rhythm obtained in many of the cases, I regard as not due to the action of pitch as a rhythmical factor, but as due to the variation in the measurements of a quantity whose true value is zero. Zero is the value most frequently found, and the one about which the others are rather symmetrically distributed. It is true that the difference between the averages for $A-B$ in the case of the same subject for different types of rhythm is great enough, in some instances, at any rate, to have some significance. There are probably certain constant errors entering in, which were different for the different types of rhythm. Such may have been certain specialized habits of motor response, or of attention, or errors in equating the intensities. Also errors in measurement and readings on the part of the experimenter. These might be different for each type of rhythm, as in nearly all cases all the work done with any one combination of pitches by any one subject was finished before work with the next combination was begun; and in any case, it was not difficult for the subject to recognize the type of rhythm used, as the three pitches used were all widely different. But the fact, that the differences existing between the quantities $A-B$ are not wholly without significance, makes it none the less certain, in view of the fact that the sign of $A-B$ changes for the different types of rhythm, that the rhythm is not determined in a reliable or constant way by the pitch differences.

I have used only seven different combinations of pitches and these all very simple ones. It might be argued that my conclusions would not hold, were other combinations used. While admitting this possibility, I wish to emphasize the

fact that I have used harmonious combinations of pitches as well as some that were not especially harmonious, with the same negative result in both cases. It seems to me extremely doubtful whether the more complex melodies would be found any more rhythmical than the combinations of pitches that I used, it being remembered that differences in duration as well as in real and apparent intensity are to be considered as having been entirely eliminated.

## Conclusion.

Pitch, intensity and duration can no longer be looked upon as *stellvertretende* factors, any one of which may be substituted for either of the other two. The rôle of each in rhythm is radically different. Intensity has a group-beginning effect: duration, a group-ending effect: pitch, neither a group-ending nor a group-beginning effect.

# EDITORIAL

## WILLIAM JAMES

No one who knew William James can ever write of him in a wholly objective and dispassionate spirit. Nor can one cherish any hope of tranfusing into words the abundant richness of his wonderful nature. Any just estimate of the man would require for its accomplishment a soul as pure and fine and brave as his own. The final valuation of his work must be confided to another generation. These lines are devoted to a few brief reminiscences, and particularly to such as may remind us of the profound changes in our psychological thought and practice for which we are primarily indebted to him.

To be sure at the time of his death, James was generally regarded as a philosopher rather than as a psychologist. And a philosopher he was in the best sense of the word, a devoted and courageous seeker after the deepest truths of life. But he was also a psychologist, easily first among his countrymen, and to emphasize this fact is the peculiar privilege of this REVIEW, with the first number of which his name was associated as an editor.

We may begin our reminiscences with the publication of 'The Principles of Psychology' in 1890. Prior to that James had been writing occasional brilliant articles and essays, some of which appeared later in modified form as chapters in the book, still others coming to life again in the volume entitled *The Will to Believe*. But until the appearance of The Principles his influence was on the whole secondary and local. With the publication of this great work, which the writer believes to be altogether his most valuable and lasting monument (it has in it most distinctly the seeds of all his later philosophical development) a profound and radical change came over the scene.

Who does not remember the sense of glowing delight with which we first read the pages of the big, cumbrous, ill-bound and rather ill-printed volumes? It was like inhaling a rare, pungent mountain air, vital, bracing and almost intoxicating. To many of us of the younger generation the book was assigned as a text. We read it as one reads the most fascinating tale of a master—spell-bound and transported and yet withal feeling ourselves acquiring new powers, and gaining command of pregnant thoughts. Scores of other readers fared as did we and so it came to pass that almost over night James

became the recognized fountain head of the most original and most vigorous psychological thinking in our country. To get the perspective one must recall the conditions of our literature at that time.

In 1887 Ladd published his treatise on *Physiological Psychology* and Dewey his text-book on *General Psychology*. Two years later came Baldwin's *Senses and Intellect*. Signs were therefore not wanting of an intellectual ferment at work. But Ladd's volume was too technical to attract general attention, and the other two books, although favorably received, suffered under the suspicion of being philosophy rather than pure psychology, added to which both were addressed to the needs of college students and followed rather rigid lines of exposition. Prior to the appearance of these works, American students had been nurtured almost exclusively on Porter, McCosh, Hickok, Wayland, Haven, Hopkins, and their various followers. Not that all these authors deserve in any sense to be classed together, but simply that they represented in various degrees scientifically archaic and outworn methods in psychology, which were nevertheless still current in our colleges. At such a critical period appeared the epoch-making *Principles*.

It is difficult to appreciate how much that is now familiar and commonplace in psychological writing was introduced by James. But the simplest process of comparing the authors last mentioned with the writings of to-day abundantly exhibits the transition which has occurred and for which James more than any other one man is directly responsible.

Perhaps the most fundamental of all the doctrines he introduced and popularized was the physiological conception of habit as the basal principle of mental organization. He not only made this pivotal in his account of the associative processes of the intellect, but he exhibited it as the central feature in the development of the will. When a decade later he published his *Talks to Teachers*, he took it up again and made it cardinal for educational practice.

If the principle of habit may in certain ways be regarded as the most important interpretative principle running through his work and the one which enlisted the widest and most immediate interest —and general assent—it must in other ways share the honor with its complementary principle, the cerebralistic hypothesis of the conditions of thought.

This hypothesis is of course in no sense an invention of William James. Indeed, in many ways it simply formulates facts well known to the veriest way-farer. But no previous English writer on psychol-

ogy since Hartley's day had espoused this view so explicitly and at the same time made such constant practical use of it. James turns to it at every point for the explanation of mental phenomena, a practice which caused him to be branded by the orthodox as a dangerous materialist.

He shared with Lange the honor of revolutionizing the current view of emotion. The exposition of his theory rescued the subject at a stroke from the dull routine of purely descriptive psychology and, notwithstanding a constant fire of acute criticism, his position has substantially held its own up to the present day.

Following closely in the footsteps of Darwin, he made instinct an essential part of the study of the human mind in a way no other psychologist had done. He took a large, flexible, dynamic view of instinct which gave it a place in the very forefront of human life and so of human psychology, instead of relegating it to the limbo of 'left-overs' from our animal ancestry for which apologies must be made and moralizing indulged.

He wrote perhaps the only thoroughly entertaining account of reasoning that we have in the English language and yet managed to make clear much of what is technically most essential to know, if one would understand the actual workings of the mind. Despite their admirable analytic fidelity to detail, one has only to compare with James' chapter on 'Reasoning' in the *Principles*, certain of the contemporary German analyses of the process, to feel that he is in reality closer to the pith of the matter than they.

His account of space perception established the subject on a level which it had never before reached in modern English writings and thereby rendered a service of lasting value. It still remains, twenty years after it was written, one of the most valuable of store-houses for all the fundamental phases of the subject.

James made a conscious and systematic struggle for the recognition of the vague, the fugitive, the transitory, in consciousness, as over against the 'block-house' scheme of mental organization. This tendency appeared most clearly perhaps in his celebrated chapter on the Stream of Consciousness, and more especially in his doctrine of 'Feelings of Relation' in which we have the first adequate insistence in contemporary writing on this aspect of consciousness.

His conception of the self as a hierarchy made a deep impression, not so much perhaps for its intrinsic novelty, as by reason of the peculiar picturesqueness and force of his description and his pungent sallies at the expense of popular complacencies.

He made much wider use than had any previous English-speaking

psychologist of materials drawn from the pathological side of mental life. This was in part no doubt a reflex of his medical training, in part was due to his intrinsic interest in the unusual. This trait appears very strikingly in the selection of material for his great work on religious experience.

No one thing so radically divided professional opinion of James as his support of the Psychical Research Movement. In England the society began its career under auspices in many respects most favorable and in all respects scientifically respectable, not to say, eminent. But in this country, James for years stood almost alone among men of high intellectual repute, and although he was eminently conservative in his estimate of the results of the work of the society, he nevertheless committed himself to belief in certain mediumistic phenomena in a way which seriously offended many of his professional colleagues. At best, they regarded him as a man whose judgment could not be trusted, at worst as an unwitting backer of quackery and fraud.

His position on the issue was all of a piece with his insistent and never failing protestantism, his passion for fair play, and a just hearing for all sides of every question. He was never afraid to make a mistake if only it were honest, and he started off on a new tack when he found himself in error, as though fallacy were a regular part of the day's work. He had a wholesome contempt, which I have heard him more than once express with characteristic vigor, for the pose of infallibility and essential omniscience assumed by certain distinguished scholars. It seemed to him at once petty and contemptible.

Whether or not we agree with his view of psychic research, it will be a thousand pities if James' attitude in the matter should fail to teach us of the younger generation the high rewards of honest independence and the insidious dangers of moral stultification which lurk in a lethargic or a cowardly intellectual conformity to ruling creed, whether of church or of science.

In this matter, as in the others to which we have referred, we may gratefully look for his most enduring contributions not so much in the specific doctrines which he taught and defended, as in his splendid spirit of eagerness for the truth, his open-minded willingness to find this truth in humble and out of the way places, and his dauntless courage in proclaiming his faith in whatsoever he found worthy.

With few men could an inventory of his accomplishments, however impressive, be so entirely and grotesquely inadequate as with James. It has been a favorite comment upon his writing that in his case the 'style is the man' in an extraordinary measure. And the saying is

true, but what it leaves unsaid is more than what it utters. His personality was fascinating and magnetic to a degree which his writings hardly indicate. This was perhaps in part due to the wonderful play of light in his eyes, in part to the singularly sweet and resonant tones of his voice, to say nothing of the racy picturesqueness of his words. But inwardly no doubt it sprang from the unsounded wells of sympathy in his nature which made every human creature a thing of at least passing interest to him.

As a teacher he was especially sympathetic and stimulating. He overestimated the capacities of many of his students most seriously, or at least, appeared to do so, but this only put them on their mettle to make good his estimate. What he did in this way to encourage his own pupils, he did more widely for struggling talent wherever he chanced to note it. No brochure so obscure, no writer so unknown, but that if James detected any spark of promise, he sent a word of appreciation. How much such encouragement meant to many a young scholar toiling for recognition, no one will ever know. But the sum must be large beyond belief.

William James was that rarest of human beings—a great man who was also simple, kindly, brave and true. His memory will always be with us as an inspiration and a benediction.

JAMES R. ANGELL.

THE UNIVERSITY OF CHICAGO.

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